

ERTS — Technological Success, Scientific Failure ?

INEQUALITY in the distribution of research funds does not always matter much. The mathematician may need no more subsistence than a living wage and some money to buy pencils, whereas the biologist may want assistants, laboratories and electron microscopes. No-one, certainly not the mathematician, would begrudge that inequality. There are others, however, which are less justifiable and they have sprung up with more regularity in recent years when the white heat of the technological revolution has been igniting, with the aid of vast funds, projects which in previous times would have had no cause to take light. It is now necessary to take a very hard look at some of the products of this earlier largesse. Consider, for example, NASA's Earth Resources Technology Satellite for which we must accept the inelegant acronym ERTS.

There can be no doubt that aerial observation of the Earth is in some circumstances a desirable tool for a wide range of disciplines. Photography from aeroplanes on specific missions has clearly solved many important problems. Military aerial reconnaissance from satellites, such as is regularly practised by both the United States and the Soviet Union, has established itself as contributing in a surprisingly effective way to stability between the super-powers. Meteorological satellites have shown their worth in a number of applications. Will ERTS observations prove equally useful?

The ERTS launched in July 1972 carried three devices—

- a data collection system for relaying data from scattered ground sites back to base
- a return beam vidicon consisting of three cameras operating in different parts of the visible spectrum, and
- a multispectral scanner producing continuous strip images for four colours, one in the near-infrared.

The satellite is in polar orbit and crosses the equator on each orbit at 9.30 a.m. local time, when viewing is said to be most favourable. The same spot is rephotographed once every 18 d.

How useful is all this? And if useful, is it worth the money? Such questions are extraordinarily difficult to answer. At one time Hertz's work on electromagnetic waves was thought to have no uses; on the other hand no-one was investing tens of millions of dollars in it.

The operation suffers from one major defect—it is too generalised. It is not possible simultaneously to please a large constituency and provide each of them with what they need. Furthermore it is by no means clear that many of the users of the ERTS data can make much of it beyond the 'gee-whizz' stage. When more detailed work is attempted the resolution is too poor (objects less than a few tens of metres across may be invisible), the time of the photograph is unsuitable for the problem or the effect is not suitably sampled in 18-d intervals. For instance one of the uses of ERTS has been claimed to be flood assessment, and indeed rather good photographs of flooding on the Mississippi have been released. But if there is a

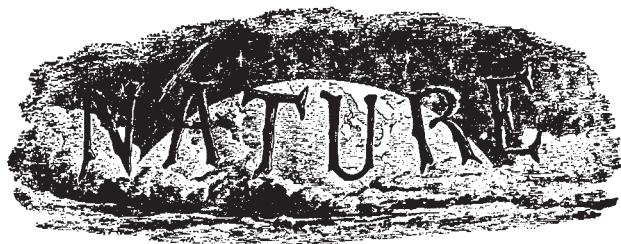
need for rapid assessment, ERTS may not be available for many days and even then may be blotted out by clouds, whereas a tailormade aircraft survey could do the job quickly and repeat it at will. It has been claimed that ERTS will assist in pollution monitoring, and there is no doubt that it can do so in certain circumstances, but is it necessary? Maybe chimneys can be seen smoking, and rivers being polluted but this is a most expensive substitute for ground or near-ground work, and indeed one which only records visible or near-visible pollution on a largish scale and then only at predictable times and when there is no cloud cover.

Much has been made of the geological uses of ERTS; indeed the publicity has tended to imply that geology is the study of lineations, and lineations can mean metal deposits. One cannot imagine why any geologist would be prepared to make a partial map of his area—partial because of the finite resolution and because structures not visible at 9.30 a.m. would not be recorded.

Perhaps ERTS is valuable in agriculture and land-use surveying. If so, is it demonstrable that *ad hoc* low-level surveys would not have served the same purpose equally well? Unfortunately it may be easier for NASA to spend tens of millions on ERTS than for some agricultural authority to spend a small fraction of this on a more conventional survey.

It is not possible to examine all the hundreds of ERTS projects and to come to any reasoned conclusion that the whole ERTS operation is poor value or scientifically ineffectual, and indeed this very proliferation of projects can be used by apologists for ERTS. Nevertheless one has the distinct feeling that no matter where one looks ERTS does not make good sense as a scientific programme of high priority for the seventies. There are signs that NASA may feel the same way. The next satellite, planned for this year, has been delayed to 1976. Maybe it should just be forgotten about.

100 Years Ago



WE learn from the *Bulletin International* of the Paris Observatory, that Lieutenant Parem and Dr. Wykander, while passing the winter of 1872–3 on the coast of Spitzbergen, made a series of spectrum observations on the Aurora, and determined seven different spectral lines, which, according to Wykander, are identical with the spectrum at the bottom of the flame of a candle or petroleum lamp.

From *Nature*, 8, 536, October 23, 1873.



Nobel Recognition for Ethology

Professors R. A. Hinde and W. H. Thorpe of Cambridge University Sub-department of Animal Behaviour assess the significance of the award last week of a Nobel Prize to three animal behaviourists.

THE award of the Nobel Prize for medicine and physiology to Karl von Frisch, Konrad Lorenz and Niko Tinbergen marks the full emergence of the study of animal behaviour from one of the less respectable corners of natural history to the forefront of the biological sciences. Before the 1930s most studies of animal behaviour were conducted under conditions which bore little or no relation to those for which the animal was adapted, and yet were thought to provide material for almost limitless generalisations. Alternatively they involved field studies of individual species, sometimes conducted with great insight but with little apparent generality. Konrad Lorenz's great contribution was made possible by a quality which students of behaviour had come to look upon as a mere nineteenth century virtue—the ability to observe with insight. Through this he acquired an extraordinarily detailed knowledge of a few species and a broader knowledge of many others. This permitted him to attempt generalisations which began to weld the biological studies of behaviour into a coherent whole.

As a biologist his early interests were in the evolution of behaviour, and he focused on stereotype movement patterns as providing material for the comparative studies of closely related species. This simultaneously involved him in studies of internal causation on the one hand and ontogeny, including perceptual development and imprinting, on the other. Previous to Lorenz's work, the 'problem of instinct', though raised acutely by Darwin, had been largely ignored because the terms used in its discussion were ill-defined or meaningless and their relation to each other obscure. Lorenz, using concepts derived from a variety of sources in the United States and Europe as well as new ones of his own, produced for the first time a general system of behaviour with reference to both 'instinctive' and 'learned' aspects which showed at what point observational and physiological

work could most effectively be directed. This released a torrent of research in the following thirty years. Inevitably there are some issues on which Lorenz turned out to be incorrect. His energy model of motivation, inherited from Freud and McDougall, which led him into insecure speculation about the nature of aggression is now generally seen to be misleading; his early attempts to divide behaviour into innate and acquired elements he later refined into a dichotomy of sources of information for adaptedness; and he has often been too ready to generalise from animal to man. On such issues as these popularisers have done further harm by misunderstanding and dramatising the issues. But it is indisputable that Lorenz imparted a momentum to the study of behaviour it has not since lost.

Lorenz is an acute observer. But he has never been an experimentalist. Both von Frisch and Tinbergen have used an experimental approach but neither has felt that the only way to do experiments on behaviour is to bring the animal into a carefully controlled laboratory setting—rather the experiment must often be taken to the animal in its natural environment. Von Frisch's major behavioural work arose from the manifest absurdity of C. von Hess's claim that bees were colourblind. The claim had been based on a grotesque experiment in which bees had been confined in darkened boxes with variously coloured exits. Because they chose at random they were assumed to be colourblind. Since bees appeared to use colour in feeding, von Frisch started a careful examination of their foraging behaviour. This led him to discover the now widely known communication system of the bee dances. By the very nature of the problem he was forced to experiment in the field. Only because he did so did he discover the dance.

Tinbergen's early work was guided by the great tradition of natural history in Holland, and primarily involved seal studies. In 1938 Lorenz invited him to his laboratory in Austria, and the year they spent together, rudely interrupted in 1939, was crucially important. Tinbergen moved to England a few years after the war.

Before he left Holland, Tinbergen had already initiated three major lines of work which have continued to occupy him and his students. One was concerned with ways in which predatory insects orient in their natural environment, and another involved studies of the life history of the herring gull. The third and perhaps the best known was made possible by the choice of a species al-

most ideal for experimental studies—the three-spined stickleback. Its great merit arises from the fact that it is possible to create in a laboratory tank a near natural environment where experiments can be carried out with great precision. Its choice by Tinbergen was no mere matter of luck.

Perhaps the most important landmark in this field of science was Tinbergen's publication, in 1951, of *The Study of Instinct*. This brought together the products of his earlier field studies, of his studies of sticklebacks, and of his collaboration with Lorenz. Perhaps even more important was the way in which it delineated the problems comprised in behaviour study—not only the problems of its immediate causation, but its development in the individual, its evolution and its adaptedness. From this point on there was both a central body of theory and a basis of hard facts from which to proceed—and also some indication of how to proceed, with careful observation preceding experiment, and resynthesis accompanying analysis to avoid the reductionist fallacy. Although some parts of the book came in for criticism, it proved to be a major inspiration for behaviour studies through the fifties and sixties.

Three qualities of Tinbergen's work require special mention. First it is he who really perfected the field experiment. While observation always precedes experiment in his work, the one merges into the other in a manner that permits a biologically valid analysis. Second, by applying this method Tinbergen has opened up a previously almost totally neglected set of problems—the adaptedness of behaviour. The issue is raised by comparative studies; just why is it that closely related species differ in their behaviour? By careful field experiments Tinbergen has demonstrated the adaptive significance of many aspects of gull behaviour and shown how the different behavioural characters of a species fit together to form an integrated and adapted whole. Finally, in a subject which too easily lends itself to woolly thinking, Tinbergen's clarity set standards which all can now attempt to emulate. The importance of this cannot be over-emphasised in an area where limited lessons about our own behaviour are to be learned, but slick generalisations are easy.

This award has not been made for the solution of a particular problem in an established field, but for the creation of a new science—ethology, the biological study of behaviour. Konrad Lorenz, Karl von Frisch and Niko Tinbergen have nurtured it through the storms and controversies that accompanied its emergence, and richly deserve the recognition they now receive.

The Life Game : Expectations Disappointed

Professor J. H. Edwards, of the Department of Human Genetics, University of Birmingham, reviews *The Life Game*, shown on BBC TV last Saturday.

NIGEL CALDER, writing in *Nature* last week (245, 293; 1973) stated the aims and the means, and clearly defined the limitations of having to keep millions of viewers awake for over two hours by conducting them, in Faraday's words, "over paths strewn with flowers".

The path was evolution, the flowers the living evolutionists, captured on film and tape and pinned down against the background of their studies. These specimens were brought from as far as possible, preferably from the USA. The inscrutable East was represented by Kimura, who speaks English well, speaking Japanese while awaiting equally inscrutable printed output from a computer.

We may ask, as of a many-authored book, are the parts accurate, are they lucid, do they entertain, are they relevant, is there a central theme, and is it worth the price?

The standard of accuracy was high, and the scrupulous procedure which allowed this unusual achievement was made clear in Nigel Calder's prologue. Lucidity was variable: the static models which furnished the central lecture theatre were rather disappointing, at any rate in black and white. The haemoglobin molecule was not obviously compounded of sub-units, nor were these obviously single stranded.

The DNA spiral, like a rungless ladder, lacked its basic asset. The studio floor showed only the end of evolution and lacked a time scale. These limitations were by no means compensated for by hiring an elephant which stood throughout the programme with dauntless patience.

Some points which needed visual phylogenetic aids, including the gulls of the world, the flies of Hawaii, and the protein sequences of Fitch, were handled in mere words. The scientists did not seem to have access to the sort of models the BBC seems able to fabricate so expertly, or even to have access to a

blackboard. This dominance of editor over scientist—or patron over poet—lost the essence of science, the child's view, the triumph of simplicity over complexity, and the eureka moment. This was not a programme to make viewers rush back to school to change their courses and fit themselves for the frontiers of ignorance.

The most lucid exposition, and the most beautiful, was the dance of Swedish school children dressed as bases and amino acids. Why the BBC went to Sweden was not explained. Unfortunately for lucidity the aesthetic challenge of a revolving spiral somewhat obscured the distinction between replication and transcription and no static models were used to clarify this basic dogma. Mutation was handled with elegance and mystery, but not in the most conventional way, which would have been easy with so many ingredients for childish error. Lucidity did not always flow from the laboratory: in some the 'teams in tidy rooms' relentlessly pursuing truth or rejecting hypotheses suggested the analogy of a war correspondent coming back with films of a regimental parade.

Entertainment value was difficult to assess. The scientists themselves were not as entertaining as they might have been in a less constrained environment: their occasional demonstrations of swimming underwater, piloting aeroplanes, and the distress felt by one over Vietnam, did little to sweeten the pill.

The central theme, which I take to have been 'chance or necessity' provided a far from constant and coherent thread: some parts, including the differentiation of Hydra and chick, seemed of little relevance in spite of their intrinsic interest and excellence. The games theory approach was ingenious but its lucid introduction by Maynard Smith came almost at the end. *The Life Game* differed from *The Violent Universe* and *The Restless Earth* in being less observational and more experimental in content and this made it more difficult to handle as a spectator sport under the dominance and guidance of a director, and even more difficult under a changing cast of directors.

Notwithstanding claims to the contrary, nothing which was expressed required computers: the possibility of displaying synthetic evolution in real time on computer consoles was not realised although this would seem ideally suited to television. Nor were sounds exploi-

ted: the calls of neighbouring gulls, or even the courtship noises of the flies, might have broken the silence or the music.

Finally, was the expense of this extravaganza justified? Unfortunately the cost seems a trade secret: it would be of interest to know how it compared with its illustrious predecessors on earth or sky, or with Bronowski's one-man tour, or with the annual budget of the Open University series, or even with *War and Peace* or *The Forsyte Sage*, those other epics of chance and dissolution.

An apparently reasonable question is why did our national broadcasting company spend so much on travel when many of the problems, and the experts, were at hand? When the haemoglobin molecule was unravelled in Cambridge and both negro immigrants and Africans are at hand, why go to New York to photograph a family being screened for sickle cell disease? When the natural protein variabilities were first extensively studied on man in London, why demonstrate it on flies in Chicago? When Edinburgh is a world centre for repetitive DNA why go to film this in California? When one of Kimura's leading mathematical critics is in Brighton, why track down his major non-mathematical critic in the USA? Why film children's games in Stockholm, rather than Shepherd's Bush? The answer is that the programme is being jointly produced by many countries, each of which presumably needs at least a slice of the action. British taxpayers, unaware of this, might have wondered whether the expense was justified.

The problems are difficult, as Calder points out: it is easy to find fault but not easy to succeed in the challenge to retain uninformed viewers for two and a half hours on Saturday evenings. To have avoided either error or sensationalism is a considerable triumph of integrity over temptation. The investment is, one hopes, not fully exhausted as the many items each justify a programme in their own settings and some, including the Galapagos, have yet to be seen. The reviewer was ill-suited to judge the educational value, lacking the appropriate ignorance of the field. His expectations had been aroused before the programme by David Attenborough's television programme on the giant caves of Borneo earlier that week and by Nigel Calder's prologue in *Nature* on the preceding day, and he was disappointed.

OLD WORLD

Hope at Last for Civil Servant Scientists

At long last the Government has announced that it has referred to the Pay Board the question of what criteria and methods should be used for determining the pay of the science group within the Civil Service. The government's intention was first made public last April but it has taken six months for Whitehall actually to present the issue to the Pay Board.

Mr William McCall, General Secretary of the Institution of Professional Civil Servants said this week that the position of civil service scientists is now much worse than it was at the beginning of the present campaign to bring the salary of government scientists into line with the administration and technology grades. He also said that it is imperative to get acceptable recognition for government scientists and that it is essential to solve the long term problems of parity with other grades.

The problem "merits being designated a special case" under Phase Three of the government's pay plan, said Mr McCall, and it will be quite inequitable if the government does not accept that.

The Pay Board will not determine the pay scales of government scientists but what it will decide are the methods and criteria for determining pay. In the past, the pay of government scientists has been determined by a process known as Pay Research under which the salaries of government scientists are compared with the salaries of scientists working outside the service, for example in industry. Such a system, according to the IPCS, is unfair as there are so many scientists working in government (about 17,000) that it is inevitable that salaries outside the civil service are to some extent determined by the salaries paid by the government. Therefore the system breaks down. The institution would much prefer to see equity developed within the civil service and the pay of the scientists matched to the pay of the administration and the professional and technology grades. An Administrative Principal working outside London now commands a salary at the top of the scale of £4,708, and a Professional and Technology Principal's maximum is £4,760. The Principal Scientific Officer is very much at the bottom of the league table earning, at most, £4,387. What is distressing to the scientists and their union officials is that as recently as 1969 the maximum salaries of all three grades were comparable.

The issue first came to a head in the summer of 1971 when massive demonstrations in favour of an increased salary for government scientists occurred. Mr McCall at this time boldly stated that there would never be another pay exercise for scientists in government employ in Britain. The issue, at that time, resulted in an independent tribunal being set up which brought in a verdict which did not wholeheartedly favour the scientists' case. But it offered them more than the government was prepared to do following a pay research inquiry.

Since then the situation has developed into a stale mate. The IPCS pressed for an independent committee to determine the criteria by which the pay of government scientists should be determined. The government prevaricated until September 1972, when the first

phase of the current prices and income strategy was introduced, which effectively brought negotiations, for what they were, to a halt. Finally in April of this year the government agreed in principle to refer the matter to the then newly set up Pay Board.

A decision is expected from the board within two months. The procedure will be that the IPCS and the Civil Service department will both present their cases and both sides will be bound by the board's decision.

Mr McCall says that he is confident that the institution's case is so strong that there is no likelihood of the board recommending a return to pay research. But if the worst comes to the worst the institution will accept the Pay Board's decision, provided the official side does likewise.

Uranium Resources

URANIUM demand for nuclear power programmes, currently 19,000 tonnes of metal a year, will treble by 1980 and quintuple by 1985. By then 100,000 tons per annum will be needed, according to a report published by the Organization for Economic Cooperation and Development. "It is therefore essential that urgent steps be taken to increase the rate of exploration for uranium so that an adequate reserve may be maintained".

The report also gives a fuller guide to Britain's uranium deposits than has been available to date. The Institute of Geological Sciences undertook a five-year survey for the United Kingdom Atomic Energy Authority in 1965 to try to find uranium in Britain. Although the fact that the search was successful has been announced, the quantities discovered have not been disclosed. The report states that widespread but low-grade enrichment was found in Caithness and Orkney in Devonian sediments, with local enrichments providing "a few thousand tons of U_3O_8 probably coming within the \$10-15 per pound price range". Investigations in the southwest of Britain have revealed concentrations associated with wrench faults in proximity to Hercynian granites. The report suggests that some structures "show evidence of continuity sufficient to indicate resources of the order of hundreds of tons of U_3O_8 ".

The report—a joint study undertaken

by the Nuclear Energy Agency and the International Atomic Energy Agency puts reasonably assured world resources of uranium that can be mined at \$10 per pound at 866,000 tonnes with a further 916,000 tonnes of estimated additional resources that can be mined at the same price. Once the price increases to up to \$15 per pound a further 680,000 tonnes of reasonably assured resources are thought to be available, along with 632,000 tonnes of estimated additional resources.

Although these figures are appreciably larger than those produced by the last OECD report on uranium (published in 1970) the agencies note that "current uranium prices are generally not quite adequate to permit the necessary exploration or to provide the increased expansion of production capability needed". The agencies also point out that in arriving at their estimate the possibility that many countries may accelerate their nuclear programmes because of an energy shortage "was not taken into account".

The agencies warn that some means must be found to ensure that the production levels required are achieved so as to avoid an unstable market in the 1980s.

No solutions are suggested, but a further warning is issued about the demand for uranium enrichment plant. Existing and planned capacity over the next decade is likely to be adequate until the early 1980s, but when additional capacity is needed it will be needed very quickly indeed, to meet a huge leap in demand.

Short Notes

Geophysics Comparison

AN interesting, even if outdated, comparison of the systems of teaching and research in Earth science departments in Britain and the United States is to appear in the next issue of *Comments on Earth Sciences: Geophysics*. In 1970 a questionnaire was distributed to 25 British based Earth scientists who had spent a substantial part of their careers in the United States. The questionnaire asked these scientists to grade, on a scale of -2 to +2, different aspects of Earth science departments in the two countries. A -2 mark implied that the system in the USA is very superior whereas a +2 mark implied the superiority of the British system. It comes as no surprise to most, perhaps, that the system in the USA came out in most aspects to be better, in the opinion of the 25 scientists polled. In particular, it was felt that in the United States there is much more encouragement and opportunity for the scientists to present papers at professional meetings. The British system is only considered to be superior in a few aspects, one of which was that the quality of students, at least in 1970, was better in Britain. Surprisingly, there also seemed to be more technicians available in Britain than in the United States.

Professor Keith Runcorn of the University of Newcastle on Tyne, who with Professor N. D. Watkins and Dr Peter Smith organised the questionnaire, said last week that it is important to realise that the opinions reported did refer to three years ago and that the tight money situation in the United States might bring out different opinions if the poll were carried out now.

The questionnaire has, however, had one effect. The Education Committee of the Geological Society of London and the Committee for Higher Education in the Earth Sciences are jointly organising a symposium next May on post-graduate teaching in the Earth Sciences. It is proposed to discuss the implications of the questionnaire at this meeting. It would be of real value if a more up-to-date questionnaire could be made available by then. It will inevitably be asked to what extent the financial squeeze in the United States and the growing awareness of the defects of the British system of post-graduate teaching has affected the situation in the past three or four years.

Student Grants

BOTH the Department of Education and Science and the Committee of Vice Chancellors and Principals had something to say about student grants last week. The committee issued some appropriate advice to the department about student grants. The department,

on the other hand, increased the allowance in respect of the first child of a student who has no wife or husband, from £2.02 to £4.81 a week.

The department points out that its main awards scheme is designed to "discourage students from taking on commitments—such as marrying and raising a family—after their course has started". Under this scheme many students find that they are not eligible for dependent's allowances as these are only provided for students who are both married and over 25 years of age (21 for women) or who have supported themselves for three years before their course started. The students who are left out in the cold by these conditions can apply to their local education authorities but if the LEA turns down the request the student then has to apply to the Supplementary Benefits Commission. The new arrangements will ensure that uniform treatment is accorded such students in future.

There is no doubt that the Committee of Vice Chancellors and Principals feels that students are under-supported. It points out that in recent years the government has begun to threaten the principle that grants should be available to cover the basic maintenance costs "by fixing grants which fall short of basic living costs".

This, says the committee, is in direct conflict with government policy which requires universities to make economic charges for board and lodging. In this respect the committee points out that student protest directed against the universities "is misconceived". "It wastes time and money on conflicts which are not created by universities and which are not in their power to resolve."

The advice from the committee to the DES is that student grants should be fixed at realistic levels and reviewed annually to take into account changes in the cost of living. Such statements are bound to find favour with students although the department might well find the exuberance of the committee embarrassing.

Scientific Information

THE Office for Scientific and Technical Information (OSTI) after eight years within the Department of Education and Science will be transferred to the *aegis* of the British Library next April. Once this has been done OSTI will extend its activities, which now include the natural and social sciences, to cover also the arts and the humanities.

But there are some aspects of OSTI's present work which will not go along to the British Library next April. Responsibility for the coordination of national effort in scientific and technical information will remain within the DES, as will the allocation of postgraduate training awards in information science, while

the Science Research Council will take over the support of critical data compilation.

Daresbury Decision "imminent"

A DECISION from the Department of the Environment on whether the nuclear structure facility planned for Daresbury can be built is "imminent" a spokesman for the Science Research Council said this week. A public inquiry on the facility was held in July this year at which Professor Alick Ashmore, Director of the Daresbury Laboratory, gave evidence along with a number of his staff and other SRC witnesses. Other witnesses who spoke for the project, which will provide the SRC with a 20 to 30 million volt electrostatic accelerator, included the Cheshire County Planning Officer.

The inquiry resulted from Mr Geoffrey Rippon, Secretary of State at the Department of the Environment, calling the plans in, after Cheshire County Council had approved them. Objections came chiefly from the Daresbury villagers as the proposed accelerator's tower would be seen from the village, particularly from the church. Feeling was that with Runcorn and Warrington both expanding fast the Daresbury area should be protected as a small island of relatively unspoiled countryside.

Assuming Mr Rippon approves the plans soon, only six months will have been lost as a result of the inquiry. If any major changes are recommended, however, the whole project would have to be seriously rethought. At the inquiry Dr R. G. P. Voss, deputy director of Daresbury, argued strongly on technical grounds that the accelerator should not be turned over on its side—the position used to date for most other electrostatic accelerators which have been built, for example, the 15 million volt machines built by the High Voltage Engineering Corporation of Burlington, Massachusetts. If Dr Voss's case has not carried the day, it is possible that the old arguments as to where the facility ought to be sited will have to be reopened.

Tea and Sympathy

TWO new additions to the Soviet cuisine were announced this week, a high-protein, high-vitamin content "delicatessen pâté" of Antarctic krill, and an instant tea produced from waste clippings of the tea-bush, said by the tasters to be indistinguishable from a well-known American brand made from "the usual leaf".

Credit where Credit is Due

'CONVERSION from wavelength metres to frequency kHz was by permission of *World Radio-TV Handbook*.'

Acknowledgment at the end of a *Sunday Times* colour supplement article October 14.

NEW WORLD

Energy Research Expands Rapidly

by our Washington Correspondent

THE first instalment of a massive research and development programme, aimed at opening up new sources of energy within the United States, was unveiled last week. Administration officials outlined plans to spend more than \$1,000 million on energy research and development this financial year—about \$115 million more than had originally been set aside for such activities. Moreover, as President Nixon announced in June and reiterated last week, the extra helping is simply a prelude to a programme which will soak up more than \$10,000 million by 1980.

At the end of June, Nixon promised, as part of his second message on the energy crisis (see *Nature*, 244, 4; 1973), to step up spending on energy research and development. He then dumped into the lap of Dr Dixie Lee Ray, Chairman of the Atomic Energy Commission, the problem of how an extra \$100 million can best be spent on such activities this year. Ray produced her report in a little over two months, it was reviewed by the Office of Management and Budget and the White House's Office of Energy Policy, and it was the fruit of those endeavours that was unveiled last week.

Coal research is the chief beneficiary of the largesse, for it is set for an increase of some \$50 million above the president's budget request. Projects aimed at liquefying coal will get \$30.4 million this year—nearly three times more than last year—and research into coal gasification is set for \$63 million, up from \$33.4 million last year.

Also earmarked to share in the bonanza is research and development on geothermal energy resources, fusion and solar energy.

Among other areas scheduled for large increases above the original 1974 budget request are the following:

- The development of technologies to remove sulphur oxides from fuels and stack gases, which will allow the use of high sulphur oil and coal without harming the environment; this is earmarked for an increase of \$12 million over the budget request.

- Energy conservation, which is set for a \$6 million increase, particularly for programmes expected to produce results in the relatively near future.

- Research and development on gas cooled nuclear reactors, with the bulk of the increase going to research on thorium as a nuclear fuel, and on

reactor safety; \$7 million has been added to the budget for those activities.

- The development of new automobile engines which will be more efficient in their use of fuel; such programmes get an extra \$6 million.

As usual in such matters, however, the figures put out by the Administration last week are a little misleading, and seen in relation to Congressional activities, they are not as generous as they seem.

First, the Administration claims to have added \$115 million to the \$887 million it had requested in its original 1974 budget estimates, but a look back at the figures in that budget shows that the Administration was requesting only \$772 million. The difference is explained by the fact that the definition of 'energy research and development' has been broadened to include such items as energy conservation, environmental effects and automotive research. The inclusion of such items brings the total expanded programme to the politically appealing figure of \$1,000 million.

MEDALS OF SCIENCE

Nixon Speaks

by our Washington Correspondent

IT is not often that President Nixon talks in public about science, but when he does, his words are carefully watched for any indications of the way the budgetary winds are blowing. Last week, Nixon made many interesting remarks about science, but unfortunately they received little notice because they clashed with a rather more newsworthy event—Mr Agnew's resignation. The occasion was a White House ceremony during which eleven scientists received the National Medal of Science.

He tackled an area in which his Administration has been heavily criticised by the scientific community—the inadequacy of the science budget:

"Well, the budget is a problem in many areas. I can only say, however, that in the field of basic research, when it comes to problems of energy, when it comes to problems of the environment, we must allocate a larger proportion of our national income to those areas. . . .

"What I am saying is simply this: We all know that because the United States needed a concentration on defense at a critical time, and then later a concentration on space, that this opened up broad

Second, Congress has already passed, or is about to wrap up, appropriations bills which have added between \$80 and \$90 million to the Administration's original budget request. Thus the plan unveiled last week is only some \$20–\$30 million greater than Congress has already approved.

One aspect which is causing some concern, however, is where the money will come from. As President Nixon is repeatedly telling the world, he has no intention of raising taxes, and has a firm commitment to hold federal spending to \$269,000 million this year. The question, therefore, is will the extra money for energy research come from somebody else's budget—biomedical research, for example? At a press briefing in the White House last week, Dr H. Guyford Stever, Director of the National Science Foundation and the President's science adviser, Dr Ray and other Administration officials indicated that the money would simply be added on to the total budget, but there is still considerable confusion about the matter.

new vistas in the areas of science, and this also resulted in a much greater federal contribution and the justification for it from a budgetary standpoint. But now as we turn from war to the works of peace, we must not cut back on that research.

"What we must do is channel the efforts in the field of research to peaceful uses, the field of energy [and] ecology . . . and not, of course, by mentioning these two to in any way downgrade the efforts we should make in the fields of health, education and others. . . ."

As for the Medals of Science, they were not awarded in 1971 or in 1972, and no explanation has been offered by the Administration for the lapse. This year's winners are Dr Daniel I. Arnon, University of California, Berkeley; Dr Carl Djerassi, Stanford; Dr Harold Edgerton, MIT; Dr William Maurice Ewing, University of Texas; Dr Arie Jan Haagen-Smit, California Institute of Technology; Dr Valdimir Haensel, Universal Oil Products Inc.; Dr Frederick Seitz, Rockefeller University; Dr Earl W. Sutherland Jun, Miami University; Dr John Wilder Tukey, Princeton; Dr Richard T. Whitcomb, NASA; and Dr Robert Rathbun Wilson, National Accelerator Laboratory.

NEWS AND VIEWS

And Now the Rolling Mantle

It may never be easy to come to terms with a major new hypothesis, idea or way of looking at things during its early years. For one thing, there is the purely practical point that it is likely to be difficult to see precisely what is going on. The central idea itself may be only vaguely formulated or imperfectly expressed; its characteristics and implications may be unclear or in dispute; the data required to confirm or refute speculation may be incomplete or even non-existent; and what data are available may be interpreted in different ways by different people to give conflicting conclusions. Ultimately, the idea will evolve to the point where it is either rejected or accepted as the conventional wisdom; but in the meantime the situation is likely to be confusing to participants and onlookers alike.

Such is the case with the concept of thermal plumes in the mantle; and one can understand Tozer's exasperation (*Nature*, **244**, 398; 1973) at the apparent use of the phrase 'thermal plumes' to indicate little more than 'states of mind'. Do plumes exist or not? If they exist, how do they differ from conventional views of rising magma? Are the supposed plumes fixed to the mantle or are they in motion? And if they move, do they do so randomly (with or without reference to the processes embodied in the new global tectonics), in small or large groups, or all together? Does material from rising plumes replenish the asthenosphere? How might plumes be used to infer something about, say, the possibility of polar wandering? The list of questions is endless; and none has yet been answered satisfactorily. For the time being one can do little more than fall back on the old clichés that it is early days yet and time will tell.

The state of uncertainty is well illustrated by some recent attempts to come to terms with the question of whether the supposed mantle plumes (or hot spots) move. Last year, Morgan (*Mem. Geol. Soc. Am.*, **132**, 7; 1972) concluded that relative motion between plumes in the Pacific could have been as much as 1 cm yr^{-1} , and Duncan *et al.* (*Nature*, **239**, 82; 1972), assuming plumes to be fixed with respect to the Earth's rotational axis, showed that European plate motion predicted by plume traces is inconsistent with palaeomagnetic data for the past 50 million years, and invoked polar wandering to explain the discrepancy. Minster *et al.* (*Eos*, **54**, 238; 1973) have used observations of relative velocities between plates at plate margins to construct a numerical model of instantaneous plate tectonics which is consistent with the view that plumes (with the exception of Iceland) are fixed in the mantle. Molnar and Atwater (*Eos*, **54**, 240; 1973), on the other hand, have rotated plates back to their 13 m.y. and 38 m.y. positions to show that some hot spots (especially Iceland and St Paul/Amsterdam) probably move with respect to each other. Clague and Jarrard (*Eos*, **54**, 238; 1973) constructed a rotational model for the Pacific plate based on the Hawaiian hot spot and, on the assumption that Pacific hot spots are fixed with respect to one another, successfully predicted the ages of other Pacific seamount and island chains (with the exception of two ages in the Austral chain).

A few weeks ago, however, Burke *et al.* (*Nature*, **245**, 133; 1973) showed that all hot spots cannot maintain a stable configuration, although the members of small groups of hot spots may well remain fixed to one another. It may also be noted in passing that the same authors (Kidd *et al.*, *Eos*, **54**, 238; 1973) have concluded that all plumes are not permanent anyway; during the past 100 m.y., new plumes have appeared and others have disappeared, and there are some which exist but fail to make any volcanic mark on the surface even when beneath stationary plates.

Into this confused situation, Hargraves and Duncan have now introduced (on page 361 of this issue of *Nature*) what may turn out to be a reconciling factor—the possibility of mantle motion independent of the lithosphere (mantle roll)—largely in response to a dilemma posed by their earlier work (Duncan *et al.*) and that by McElhinny (*Nature*, **241**, 523; 1973). The problem was that soon after Duncan and his colleagues had invoked polar wandering to explain the discrepancy between European plume traces and palaeomagnetic data, McElhinny used purely palaeomagnetic results from all lithospheric plates to show that the lithosphere has not moved as a unit with respect to the rotational axis for the past 50 m.y. In other words, if the European discrepancy is not to be attributed to polar wandering in its guise as movement of the whole lithosphere, what must be involved is movement of just the mantle (rather than just the lithosphere, or the mantle and lithosphere together) with respect to the rotational axis, or movement of the plumes with respect to the mantle.

By extending their analysis to more plume traces on other plates, Hargraves and Duncan now show that the discrepancy between palaeomagnetic and plume trace data is a feature of several plates and thus, ruling out lithospheric rotation as the cause, that the plumes must move in relation to the Earth's spin axis. Moreover, the discrepancies are different for different plume traces. The fact that the discrepancies are not equal suggests, on the face of it, that plumes move with respect to each other in the mantle, in which case there is no need to invoke any movement of the mantle with respect to the rotational axis. But Hargraves and Duncan have chosen to look at the problem in a different way and ask the following question. Is there any systematic motion of the mantle as a whole with respect to the rotational axis which, carrying the plumes with it, can account for the various different trace-palaeomagnetic discrepancies without destroying the stable configuration of the plumes?

The answer to this question seems to be positive, at least in the sense of least squares, for Hargraves and Duncan have been able to identify a best-fitting axis for mantle rotation which reduces the apparent motion between plumes to a minimum. In other words, accepting that the lithosphere as a whole remains fixed with respect to the rotational axis, they propose that the mantle may nevertheless rotate or roll about an axis which differs from the Earth's spin axis. As it does so, it carries the plumes with it in a near-stable configuration; relative

motion between plumes is minimal, but the possibility of some interplume motion cannot be completely excluded. Because the mantle rotates, the existence of plume trace-palaeomagnetic discrepancies is expected; and because it rotates about an axis independent of the Earth's spin axis, the magnitude of any discrepancy will depend on the distance between the relevant plume and the pole of the mantle roll axis.

It is this ability to view the apparently disparate behaviour of mantle plumes as the result of a single process which makes the Hargraves-Duncan model of particular interest. The behaviour of the eight plumes used to derive the model must clearly be consistent with it. The obvious question to ask now is whether the hundred or more other plumes can be fitted into the same pattern.

P. J. S.

Where are the Messengers Going?

It is remarkable how rapidly wonder gives way to boredom as new messenger RNAs, one after the other, appear on the biological scene. It is only five years since the first reports confirming the existence of mRNA in animal cells were published; first a note on a heterologous system using two genetic variants of rabbit globin differing in one amino acid position in the α chain from Schapira and his colleagues, then the definitive work of Lingrel and his group. Lingrel in particular realized that it is neither the purity of the cell free system that is used, nor its distance (in speciation) from the added mRNA, that is critical, but the existence of a sensitive and positive assay for the newly synthesized protein. A mixed system of mouse and rabbit or mouse and duck reticulocyte components is every bit as definitive as one using *Escherichia coli* ribosomes, provided the new protein can be clearly separated from any endogenous proteins that are made. Although purified protein synthesis systems have now been perfected which respond to added mRNAs, the crude reticulocyte lysate first used in this way by Lingrel is still a most active and favourite method for assaying new messengers.

It is interesting to note in retrospect that the only inter-specific cross which cannot be obtained easily is the very one most sought after in early work—the translation of mammalian mRNA by a bacterial cell-free protein synthesis system. Bacteriophage Q β mRNA can be translated in mammalian cell-free systems (Aviv *et al.*, *Science*, **178**, 1293; 1972) and rabbit globin has been synthesized using wheat embryo translation machinery (Efron and Marcus, *FEBS Lett.*, **33**, 23; 1973). Any messenger specific factors in translation may be quantitative rather than absolute in their effect, or involve systems such as virus-infected cells.

Using traditional techniques of sucrose gradient centrifugation several mRNAs were isolated from cells which make predominantly one type of protein—globin, of course, from several species; immunoglobulin; lens crystallins. Occasionally biological tricks come to one's aid rather than plague one—histone synthesis is particularly active in early embryonic stages, and is confined to smaller polysomes. Only a few mRNAs can, however, be isolated in this way, and some of the most interesting will only be present in low amounts in any given cell type.

Dramatic recent advances have been made in the development of techniques for the isolation of 'minor' messenger RNAs, some making use of the ubiquitous polyadenylic acid sequence and others immunologically precipitating specific polysomes. Using such immunological procedures the mRNAs for catalase, K-type immunoglobulin, ovalbumin and specific lens crystallins have been obtained. Since the nascent polypeptide chain often has the same immunological specificity as the completed pro-

tein, and antibodies can be prepared at high activity and stringent specificity, this is a method of very great potential. The technical problems are for the most part those of the immunological component of the system: ways of preventing non-specific polysome precipitation and of removing contaminating ribonucleases from the antibodies. Although each group has found its own solution to the problems, the various ways chosen would indicate that any method giving a very highly purified ribonuclease-free antibody would succeed. A good example of the use of antibodies for the preparation of ovalbumin mRNA has been described by Schimke and his colleagues (*J. biol. Chem.*, **248**, 540; 1973).

Since there are several thousand cellular proteins to which antibodies have been produced, many of defined function in cellular but not molecular biology, the scope of these techniques is now such that one can predict the isolation of many mRNAs in the near future—including some for proteins of mainly biological and developmental interest. The aberrant size of some bovine crystallin mRNAs (Mathews *et al.*, *Nature new Biol.*, **236**, 5; 1972) has focused attention back onto the size and amino acid sequences of the proteins.

Of course, only one or two per cent of the RNA found in polysomes is mRNA; the rest is mostly ribosomal RNA, involved in protein synthesis but carrying no information for the amino acid sequence of proteins. As is now well known, most animal (and probably plant) messenger RNAs, with the exception of histone mRNA, carry poly(A) sequences from 50–300 residues long at the 3' terminus. These long poly(A) sequences are added after transcription, in the nucleus. The poly(A) sequence will specifically interact with poly(U) or poly(T), forming hydrogen bonds like those found in double-stranded nucleic acids. Since only mRNA contains poly(A) sequences, ribosomal RNA will not bind to poly(U) or poly(T) attached to inert columns of cellulose or Sepharose. The bound mRNA can be eluted by lowering the ionic strength of the buffer or with formamide, which weakens the hydrogen-bonding interaction. Therefore, if the polysomes making a specific protein can be isolated free from other polysomes, the messenger RNA can be prepared completely pure of other mRNAs and of ribosomal RNA.

Alternatively, a shotgun approach sometimes succeeds: total mRNA is prepared free of ribosomal RNA, and then the fraction of the approximately correct size is taken from a gel or gradient. This is then tested in a sensitive cell-free system. Of course, such mRNAs must be less pure than those prepared with a preliminary immunological step, but such a technique can be applied to muscle (for the isolation of the mRNAs for actin and myosin, which are widely different in molecular weights), liver,

where Shafritz *et al.* (*J. biol. Chem.*, **248**, 3220; 1973) have recently identified ferritin mRNA, or similar tissues with a restricted spectrum of protein synthesis.

The discovery of reverse transcriptase, a viral enzyme which can make DNA copies of RNA molecules, has had a very considerable impact on the technology of the study of mRNA. The copies can be made very hot—up to approximately 80×10^6 d.p.m. per μg with tritium. Thus for the first time the number of genes for a specific mRNA can be accurately estimated even when the genes are present only as single copies—looking for one part in 10 million in the mammalian genome. The cDNA copied from the mRNA hybridizes to the 'minus' strand of the gene, rather than the coding strand, but the two are, of course, present at the same multiplicity. New techniques using iodine labelling of RNA, which also give very high specific activities, may allow an easy extension of these hybridisation techniques to most mRNA molecules.

Most of the mRNAs which have been looked at seem to be copied from 'unique' DNA, thought to be present in single copies—this is true for globin from several species, ovalbumin and probably immunoglobulin light chain constant region. Only histone genes have been shown to be present at a multiplicity of several hundreds, in the case of sea urchins. One cannot but surmise that the absence of poly(A) and the gene multiplicity, both of which are at this time unique attributes of histone mRNAs among the admittedly few mRNAs studied, may be in some way related to one another. But it is early days yet: only some twenty mRNAs have been identified and, of these, the genetic linkage and numericity has only been determined for some half dozen, and that but tentatively.

Once a specific messenger RNA can be obtained, what use is it, other than as a molecular biologist's plaything? The practical potential is considerable. As Friedmann and Roblin recently pointed out in a very thorough examination of 'gene therapy' for human diseases (*Science*, **175**, 949; 1972), there are still many problems to be overcome before absent or defective genes can be supplied or cured for conditions such as phenylketonuria or sickle cell anaemia. The existence, however, of messenger RNAs for such important proteins would be the first step in determining the gene sequence and either isolating the gene directly by hybridisation or synthesising it enzymatically.

Gene isolation procedures are merely an extension of existing DNA/RNA hybridisation technology. As Shih and Martin (*Proc. natn. Acad. Sci. U.S.A.*, **70**, 1697; 1973) have shown for the simian viral DNA SV40, once a gene specific RNA (for example, a messenger RNA) can be obtained in large quantities, it can be linked to cellulose chemically and will function to anchor its homologous DNA by a straightforward hybridisation interaction. Attempts are already in progress in several laboratories to extend such isolation procedures to other mammalian systems.

But before moving to the science fiction realm of transforming cells with necessary or pleasant genes, there are more mundane possibilities which have received less attention. It is well known that cells grown from amniotic fluid removed during early pregnancy may reflect specific enzymatic or chromosomal defects which can be correlated with specific human genetic disorders. Accurate chromosome mapping procedures coupled with *in situ* hybridisation of specific labelled mRNA or complementary DNA copies made with reverse transcriptase should

allow a correlation between the structural defects seen and their enzymatic manifestations.

After many years of hints from various groups of attempts to sequence specific mammalian mRNAs, the first report of success appeared in August of this year (Brownlee *et al.*, *Nature new Biol.*, **244**, 236; 1973). Immunoglobulin mRNA is a single polynucleotide chain (globin mRNA is a mixture of messengers for α and β chains) and is isolated from a tissue culture line, and therefore can be labelled to a high specific activity with ^{32}P -phosphate. Histone mRNAs, although a mixture of several messengers, can also be highly labelled in sea urchin embryos, and partial sequences for the mRNA for histone f2a₁ have been obtained (Grunstein *et al.*, reported at Cold Spring Harbor, 1973). The sequence of much of an mRNA can be obtained more easily than that of ribosomal RNA or tRNA as the protein sequence specifies approximately two-thirds of the coding bases. Interesting though the coding sequence is, the untranslated portions at the start and end of the mRNA, which are thought to contain control signals, may prove even more interesting—and harder to elucidate.

It is fitting that the final acknowledgment in this report should be to genetics, which has contributed so much of the background to modern molecular biology. By combining the amino acid sequences of normal human α globin and the mutants Constant Spring (Clegg *et al.*, *Nature*, **234**, 337; 1971) and Wayne (Seid-Akhaven *et al.*, *Blood*, 927, December 1972), it is possible to deduce an unambiguous sequence twenty-six nucleotides long, including and just following the normal termination triplet—UAA. But as so often is the case with sequences, the only remarkable thing about the sequence found is that it seems fairly ordinary. Perhaps as longer sequences become known the secondary structure features that are expected for the non-coding regions of mRNA will be found. R. W.

The Red Queen

THE first issue of a new journal, *Evolutionary Theory*, carries an article by 'the editor, L. Van Valen, entitled "A New Evolutionary Law". Few people nowadays are rash enough to claim the discovery of a new law, least of all in evolution. Nevertheless Van Valen's claim should be taken seriously.

He claims to have established an empirical law describing the rate of extinction; he also suggests an explanation. The law states that the members of a given taxonomic group have a constant probability of extinction per unit time. By a taxonomic group is here meant an order or higher category, having something in common ecologically as well as taxonomically—for example, terrestrial carnivorous mammals or marine gastropods. The members are either the genera or families in the taxon. To establish the law, Van Valen takes for each genus the dates of the first and the last appearance in the fossil record, and hence calculates a total survival time for each genus. He then constructs a 'life table', plotting the fraction of all genera in a taxon surviving for a given time against the time. Similar fossil life tables were published over twenty years ago by G. G. Simpson, who based on them a discussion of evolutionary rates.

Van Valen's innovation (apart from applying the method to all taxa for which adequate data are available)

is to plot the logarithm of survivorship against time, and to notice that in the great majority of cases there is a remarkable fit to linearity, just as there is if one plots the logarithm of the activity of a radioactive sample against time. In other words, the data suggest that the subgroups composing a given taxon have a constant probability of extinction. This extinction probability varies from taxon to taxon.

There are of course difficulties. To the argument that some genera disappear from the record not because they go extinct but because they evolve into something else, Van Valen replies that such 'pseudoextinction' is too infrequent to affect the result. More serious difficulties arise when a substantial fraction of the subgroups of a taxon still survive. In such cases, it is easy to show that even if the rate of extinction is in fact constant, living and extinct groups will give the same linear survivorship curves only if the rate of origination is also constant, and this in general is not so. Some departures from linearity seem to arise for this reason. In a few cases (for example the extinction of the graptolites, or the extinction of almost all the stony corals in the late Permian), there seem to have been real exceptions to the rule. But despite difficulties and exceptions, Van Valen has made a strong case for his law.

Any large-scale empirical generalisation about evolution is important. Neo-Darwinian theory gives a satisfactory explanation of the mechanism of evolutionary change, and can be tested against short-term events, but it does not by itself make any predictions about the large-scale features of the process. Such predictions will require the introduction of additional concepts; these will probably have to come from ecology, as was fully appreciated by Simpson in his *Tempo and Mode of Evolution*. Many previous attempts to make generalisations about the course of evolution have been purely qualitative, and have depended on vague terms such as 'advanced' or 'specialised' which can always be redefined so as to ensure the truth of the generalisation. The law of constant extinction is sufficiently quantitative to be testable (although there is inevitably room for manoeuvre in redefining the taxa to which it is applied).

If the law is accepted, it would clearly be desirable to develop a theory combining ecological and neo-Darwinist concepts which would have

this law as one of its predictions. Van Valen offers such a theory, which he calls the 'Red Queen' hypothesis; the Red Queen explained to Alice that it takes all the running you can do to stay in the same place. The suggestion is that species go extinct primarily because of competition from other related species, and consequently that each evolutionary advance made by any one species is experienced as a deterioration of the environment by all its competitors.

Unfortunately this idea, even if true, only leads to a law of constant extinction if a number of other hypotheses (not all mentioned by Van Valen) are added to it; in particular it seems necessary to suppose that a species which makes one advance is less likely to make another. But the law is important even if we do not know why it holds, and the Red Queen hypothesis may well be the correct first step towards an explanation. From a Correspondent

CELL GENETICS

Reversion during Fusion

from our Molecular Genetics Correspondent

ONE of the most useful techniques in cell genetics in the past year or so has been the fusion together of two cells, one of which lacks some enzyme which is possessed by the other, to form a hybrid in which the enzymatic activity must therefore be specified by the chromosomes of only one parent. By using cell hybrids in which the chromosomes of one parent are lost selectively during growth, and by growing the hybrids on selective media in which the enzymatic activity is either essential for survival or lethal to the cell, it is possible to identify the chromosome specifying the relevant enzyme.

A parameter critical to the success of such selective experiments is that the enzymatic activity needed for survival or fatal to it is supplied only by the genetic apparatus of the one parent originally possessing it. This demands, of course, that the chromosomes of the parental cell originally lacking the enzyme do not revert. Yet a recent series of articles from several laboratories has shown that one such mutation is readily reverted during cell fusion.

An enzymatic activity commonly used in cell genetic experiments is hypoxanthine guanine phosphoribosyl transferase, abbreviated by *aficionados* of this topic to HGPRT. This enzyme plays an important part in the metabolism of precursors to DNA, so that cells grown on medium containing the analogue 8-azaguanine die unless they lack HGPRT activity, whereas cells grown on HAT

medium (hypoxanthine – aminopterin – thymidine) can survive only if they possess the enzyme.

One cell line, the A9 strain, lacking HGPRT was selected by growth of mouse L cell fibroblasts on medium containing 8-azaguanine. When plated on neutral medium, A9 cells never (well, hardly ever, as the saying goes) revert from HGPRT⁻ to HGPRT⁺. The mutation was until very recently thus thought to comprise a deletion of the structural gene for this enzyme. When such mouse cells lacking HGPRT are fused with, for example, human cells possessing this activity, the only hybrids able to survive on HAT medium should be those which possess the human HGPRT enzyme because they have retained the relevant human chromosome. Whether the HGPRT activity in surviving hybrids is indeed human can be determined by examining the electrophoretic mobility of the enzymatic activity, which differs in man and mouse.

In a report of such experiments late last year, Watson *et al.* (*Exp. Cell Res.*, **75**, 401; 1972) tested five hybrid lines derived by fusions between the mouse A9 line and various human cells. The expected human HGPRT enzyme was found in only two of the five hybrid lines, one of which also contained the mouse enzyme. The remaining three lines possessed only the mouse enzyme. These hybrid cells therefore survived on HAT medium by some event allowing production of the mouse enzyme missing from the A9 cells rather than by substitution of the human enzyme.

In experiments using the 1R mouse cell line, also derived as a strain of L cells able to grow in 8-azaguanine, Bakay *et al.* (*Proc. natn. Acad. Sci. U.S.A.*, **70**, 1998; 1973) reported last July that fusion with chick embryo fibroblasts generates progeny cells able to grow in HAT. These cells all possessed mouse chromosomes which had suffered structural rearrangements (identified by Giemsa banding) but no chick chromosomes. The HGPRT enzyme of these survivors displayed the electrophoretic mobility characteristic of mouse.

In a further investigation of such hybrids, Croce *et al.* report in the September issue of the *Proceedings of the National Academy of Sciences* (**70**, 2590; 1973) on their studies of the survivors of fusions between rat cells deficient in HGPRT and human diploid fibroblasts possessing the enzyme. Of fifteen hybrid clones grown in HAT medium, eight contained human HGPRT, one contained both human and rat enzymatic activities, and six contained only the rat enzyme.

What factor is responsible for the production of rodent enzyme after fusion of rat or mouse HGPRT⁻ cells with human HGPRT⁺ cells? The explana-

tion suggested by Bakay *et al.* and by Croce *et al.* for this phenomenon is to suppose that the mutation causing loss of rodent HGPRT in the parental cells is located in a regulator gene, perhaps comprising a deletion which can therefore never revert. Fusion with human (or with chick) cells restores a regulator product able once again to switch on the rodent HGPRT genes. This would imply that regulator genes in rat, mouse, man and chick may specify proteins sufficiently alike to substitute for each other, a conclusion with implications of obvious importance for models of eukaryotic gene control.

An alternative explanation, however, was considered by Watson *et al.* and is supported by the results reported by Shin *et al.* in *Nature New Biology* (241, 194; 1973) earlier this year. Reversion from HGPRT⁻ to HGPRT⁺ in rodent cells may depend not upon the formation of a hybrid cell as such but rather on some feature of the fusion process. Shin *et al.* found that A9 and RAG mouse cells, both HGPRT⁻, were induced to revert at frequencies of 1.8×10^{-8} and 9.0×10^{-8} respectively by subjecting cells to the procedures used in cell fusion, that is bringing the cells to a high concentration and exposing them to a thermal shock.

These results imply that the mutation causing loss of HGPRT in the mouse cells does not represent a deletion, but takes some form which can be reverted under the environmental conditions used to form hybrid cells. Reversion *per se* cannot, therefore, be used as a test for hybrid formation, an important limitation on the use of this technique for genetic mapping. And the hybrid rodent-human cells possessing rodent HGPRT⁺ activity may, of course, survive on HAT medium because of such reversions rather than because of the presence of human chromosomes.

The immediate implication of these results for cell genetic studies is that the technique of selection for production of enzyme by one partner to make up for a deficiency in the other can be used only when it is possible to distinguish the two enzymes unambiguously, so that reversions may be distinguished from cells possessing the (usually) human enzyme. This may well limit the exploitation of this technique in future. Another implication is that non-reversion of a mutation in normal conditions of growth can no longer be taken to support the concept that the mutation represents a deletion of the structural gene. More than one apparently well established tenet of cell genetics is therefore made dubious by the results obtained so far, and the nature of the HGPRT⁻ mutation and the reasons for its unusual conditions of reversion may yet cast an important light on the constitution of the mammalian genome.

GENETICS

Environmental Mutagens

from a Correspondent

CONCERN about environmental mutagens is widespread and justified. The problem of devising valid tests for potential mutagens among the vast array of new chemicals in the human environment is an urgent one. It is also beset with great difficulties, requiring as it does extrapolation from data gained *in vitro* to effects suspected *in vivo*; from lower organisms to man; from somatic cells to germ cells; from cell cultures to cells in the normal environment of the body; from easily scored events with little relevance to the population as a whole to the significant genetic changes which are far more difficult to score simply and cheaply. In order to coordinate the efforts of geneticists and cytologists in this area, the Environmental Mutagen Society of the United States was founded in 1969 by Dr Alexander Hollaender; the European EMS was founded in 1970; the Japanese EMS in 1972; and this year (August 31 to September 1) the first international conference on environmental mutagens was held in Asilomar, California.

The meeting demonstrated the rapid and extensive progress in this new field. Topics ranged widely from mutagenesis at the molecular level (F. Sherman, University of Rochester) and the role of repair in the prevention and realisation of mutations (T. Kada, Mishima, Japan; S. Kondo, University of Osaka; B. S. Cox, University of Oxford; F. J. de Serres, National Institute of Environmental Health Sciences, Durham, USA; S. Wolff, University of

California, San Francisco; F. H. Sobels, State University of Leiden) to tests of specific drugs or pesticides. It was realised, however, that as in cancer therapy, fundamental work is necessary.

At present an urgent task is to provide industry with easily handled testing techniques and with training courses for personnel. Also important is the construction of a workable test protocol. Papers on a possible tier system of testing were presented by G. Flamm (Food and Drugs Administration) and by C. F. Arlett on behalf of B. A. Bridges (University of Sussex). Much work is directed towards validating and improving the presently recommended techniques. B. N. Ames (University of California, Berkeley) described bacterial strains that are made hypersensitive to mutagens by genetically built-in devices for easy penetration and for lack of genetic repair. U. H. Ehlring (Society for Radiation and Environmental Research, Neuherberg) dealt with the problem of correlation in mice between the easily detected but genetically not very hazardous dominant lethals and the gene mutations and small deletions that constitute one of the main genetic risks. The even more difficult problem of extrapolating from somatic cells to germ cells and from one mammalian species to another has been tackled by J. G. Brewen and J. Preston (Oak Ridge National Laboratory).

Equally important for the drawing of inferences from experimental to real-life situations are studies on the effects of dose (dealt with by D. R. Parker, University of California, Riverside, in a general way; by Brewen for rodents; by J. Patterson and D. Clive, NIEHS, for mouse lymphoma cells) and of dose

How to See the Proteins Spin

THE mobility of proteins embedded in bilayers is a preoccupation of many workers in the membrane field. It is known that in many membranes proteins are capable of rather free translational diffusion, and in these circumstances it would also be expected that their rotation would be little impeded. Cone in fact showed by flash experiments that rhodopsin rotates rapidly in the retinal rod membranes.

Naqvi *et al.*, writing in *Nature New Biology* next Wednesday (October 24), point out that any spectroscopically distinguishable species formed from a protein chromophore by electronic excitation or by photochemical reaction can be used as the basis of such measurements, as long as its lifetime is sufficiently long for rotation to occur before it decays. Experiments based on the depolarisation of an excited singlet state are in this regard misconceived, since the lifetime is no more than say 10^{-8} s, whereas in a viscous medium, rotational

relaxation times of microseconds and up are to be expected.

With the retinal chromophore of the protein in purple bacterial membrane Naqvi *et al.* find that flash irradiation produces an unknown species of 10 ms lifetime. In the membrane its absorption is strongly dichroic, and the dichroism does not decay measurably during this time. It follows that the rotational relaxation time of the protein is greater than about 20 ms, and is thus much less mobile than rhodopsin in the rod outer segment.

Naqvi and his colleagues are also examining the possibility of performing similar experiments based on triplet states which have a long lifetime, and can occasionally be observed in absorption. Experiments with eosin bound to serum albumin give the right order of rotation rates in viscous solutions. The possibility of using the tryptophan chromophore of the proteins is also being explored.

rate (B. J. Kilbey, University of Edinburgh, for *Neurospora*). The host-mediated assay—that is the scoring of mutations in microorganisms that were exposed transiently to an environment inside chemically-treated host rodents—has been the object of much scrutiny. R. Fahrig (University of Freiburg) scored gene conversion in yeast, injected into the peritoneum of either rats or mice, or into the testis of rats. Efficacy of the technique varied with the chemical used, but in general rats were superior to mice and the testis was the more relevant environment.

The finding of E. Zeiger (Food and Drugs Administration) that diet of the host may affect the efficacy of the host-mediated assay highlights the disturbing complexities of the test. Because of this, attempts are now being made to short circuit it by adding liver microsomes from rodents to the medium of *in vitro* tests on, for example, bacteria. Sobels quoted results indicating that at least some of the activations of potential mutagens that are carried out in mammalian liver occur also in *Drosophila*. Tests on mammalian cell cultures, including human ones, are constantly being perfected. One of the main problems here is to decide whether transmitted changes are caused by gene mutations. R. J. Albertini and R. de Mars (University of Wisconsin, Madison) and J. W. and J. M. Simons (State University of Leiden) gave reasons for inferring that this is true for their systems (human cells, Chinese hamster cells). New systems are constantly being developed and tested. Even if some of them should never become obligatory or even recommended for routine testing, all are likely to contribute results of general importance. No good system is yet available for tests on non-disjunction, although this is one of the most serious hazards. *Drosophila* (Sobels), soy beans (B. K. Vig, University of Nevada), human cell lines (de Mars) have all been suggested as promising in this regard.

Among the environmental hazards studied specifically are mercury (S. Nakai, Chiba University; C. Ramel, University of Stockholm), mercaptopurine (V. A. Ray, Pfizer, Groton), pesticides (W. F. Grant, McGill University), hycanthone (Clive; W. L. Russell, Oak Ridge National Laboratory; M. Shahin and de Serres, NIEHS), aflatoxin (A. Velasquez *et al.*, University of Mexico), tritiated water (A. L. Carsten *et al.*, Brookhaven National Laboratory), caffeine (Sobels; F. Palitti *et al.*, University of Rome), food additives (R. Valencia, University of Wisconsin, Madison), isoniazide (E. Röhrborn, University of Heidelberg) and bisulphite (R. Shapiro, University of New York).

The only possible direct approach to the estimation of human hazards is the monitoring of human populations for

chromosome damage or genetic variation. This was discussed by J. Neel (University of Michigan), who is engaged in a screening programme of South American Indians, with the surprising result that these tribes carry more genetic damage than does civilised man in his polluted environment, possibly as a result of natural hazards from, for example, viruses.

The relation between mutagenesis and carcinogenesis is not yet understood and not even established beyond doubt. There is, however, increasing evidence for the dual effects of many chemicals in producing both gene mutations and cancer. The use of genetic systems for the early detection of carcinogens was advocated by Ames and by O. G. and M. J. Fahmy (Institute for Cancer Research, London).

ORIGIN OF LIFE

Optical Asymmetry

from a Correspondent

THE Earth is 4.7×10^9 yr old and life is known to have existed for at least 3.5×10^9 yr; perhaps 500 million yr was available for the 'chemical evolution' believed to have preceded life. The proteins of living organisms are built exclusively from one set of optical isomers, the L-amino acids, although non-enzymatic syntheses always yield equal quantities of the two isomers. Was the choice of L-amino acids a matter of chance, or was it determined by some basic asymmetry in the environment? The first international conference on this subject was held at the Nuclear Reactor Centre, Jülich, Germany, on September 24–26, and was organised by Professor K. Wagener and Dr W. Thiemann.

F. Vester (Studiengruppe für Biologie und Umwelt, Munich) appropriately opened the meeting. He gave an entertaining account of how the idea of a relationship between parity non-conservation and optical activity was born, beginning with the discussion in Ulbricht's laboratory at Yale on the day after the report in the *New York Times* in January 1957 of the discovery of parity violation in β decay by Lee, Yang and Wu. The mechanisms Vester and Ulbricht considered (*Q. Rev.*, **13**, 48; 1959) were: (1) direct energetic interaction between β rays and molecules (regarded as negligible because of the large difference in energy levels); (2) longitudinally polarised β rays $\rightarrow$ circularly polarised bremsstrahlung, interaction with matter $\rightarrow$ optical asymmetry (this has a solid theoretical foundation); (3) direct non-energetic interaction (for example, by entropy exchange; considered heretical).

Vester's personal account dealt with the psychology of discovery, opposition from older scientists, frustration in the

face of experimental difficulties and so on. T. L. V. Ulbricht (Agricultural Research Council, London) commented that, in Kuhnian terms, there had never been a paradigm concerning the origin of optical asymmetry on Earth, but perhaps one had now reached the critical point. The early experiments were inconclusive; hope revived with Garay's results (*Nature*, **219**, 338; 1968) and the presence of research workers from eleven countries at the conference testified to the continuing interest in the field.

Recent work at CERN has confirmed the presence of parity-violating interactions even in strong interactions (discovery of the neutral current). D. Rein (Technische Hochschule, Aachen) presented the first theoretical study of the magnitude of the direct energetic interaction and confirmed that it is indeed small, at best 10^{-10} of the binding energy. The task of detecting such a small effect is formidable, but there have been a number of studies on the amplification of small deviations from racemic conditions, including two by the organisers, K. Wagener and W. Thiemann (Jülich) on cascade processes, such as precipitation from saturated racemic solutions, with some positive results.

L. Keszthely (Biological Research Centre, Szeged) has measured the lifetime spectra of positron annihilation in crystalline L and D-amino acids, and has found that the triplet intensity in D-amino acids is higher than in their L-isomers and that the triplet lifetime shows differences between the enantiomers. The helicity of electrons in chiral molecules, postulated by A. S. Garay (also Szeged) in his 1968 paper, was developed by him as the helical electron gas model. The coupling of the magnetic transition moment and electron spin in chiral molecules leads to small energetic differences between optical isomers.

Another group of contributions were concerned with statistical fluctuations from the racemic state in small and large numbers of molecules, a return to the idea that the selection of one series of isomers was a matter of chance. In a different category, E. Gil-Av (Weizmann Institute, Rehovot) described his beautiful work on enantiomeric microanalysis by gas chromatography. C. Ponnampuruma (University of Maryland) gave a brief survey of present thinking about the origin of life and its time scale, presented results of meteorite analysis for amino acids, and at the end reviewed the significance of the conference papers, concluding that a bias in the basic asymmetry of matter, as revealed by parity non-conservation, might have been amplified and so determined the choice of one set of optical isomers. More experiments, please.

NUCLEAR PHYSICS

Nuclear Sizes

from our Nuclear Theory Correspondent

THE sizes of atomic nuclei are determined by the spatial distributions of their neutrons and protons. The distribution of the protons is quite well known because the electromagnetic interaction is well understood, and many studies of electron elastic scattering and of muonic X rays have determined the proton distributions of many nuclei to high accuracy.

The distribution of the neutrons, on the other hand, is much less well known since this can only be found using the nuclear interaction, which is much more complicated and obscure than the electromagnetic interaction. Many different approaches have been used and although most of them tend to give neutron distributions quite similar to the proton distributions in the same nucleus the accuracy is an order of magnitude poorer.

One of the most promising of these methods depends on measuring the cross sections for the interaction of fast positive and negative pions with nuclei, and this has recently been used by a group working at the Rutherford Laboratory to obtain improved neutron distributions in a range of nuclei from carbon to lead (Allardyce *et al.*, *Nucl. Phys.*, **A209**, 1; 1973).

The method depends on the fact that at momenta around 1 GeV/c negative pions interact more strongly with protons than with neutrons, whereas positive pions interact more strongly with neutrons than with protons. Since the pions are strongly absorbed they interact only with the nucleons in the nuclear surface, so the ratio of the pion reaction cross sections depends on the relative distributions of the neutrons and protons in this region. Knowing the proton distribution, information on the neutron distribution can be deduced.

In the experiment the reaction cross sections for positive and negative pions were measured at six energies for C, Al, Ca, Ni, Sn, Ho and Pb. For the first three energies the pion-nucleon cross sections differ markedly but for the remainder they are almost the same, which provides a useful check on the method. A further check is provided by including C and Ca among the targets, since for these two nuclei the neutron and proton distributions are expected to be very nearly the same.

These reaction cross sections were analysed using the relativistic optical model, which takes account of the distortion of the incident pion wave as it approaches the nucleus and its absorption as it penetrates into the interior. Since only two numbers, the positive and negative pion cross sections, are

available for each nucleus, it is not possible to determine the complete neutron density distribution. Instead, several density distributions found in previous studies of nucleon elastic scattering and of bound single-particle energies were used to calculate the expected reaction cross sections; comparison with the experimental data then showed which were the preferred distributions.

This comparison showed that the new pion experimental data favour the neutron distributions with root mean square radii within 0.1×10^{-13} cm of the corresponding proton distributions. This is consistent with other work which indicates that for the medium weight and heavy nuclei the neutron root mean square radius is about 0.1×10^{-13} greater

than the proton root mean square radius, so that the neutrons extend slightly beyond the protons.

Subsidiary calculations confirmed that the pions interact only with the nucleons in the surface region and so the results give no information on the density distributions in the nuclear interior. Indeed even for the charge distribution most existing knowledge refers only to the surface region and little is known about the central region or the extreme fringe of low density.

It is satisfactory that the results are consistent with those already available and enhance their credibility but much still remains to be determined about the proton and neutron density distributions, particularly in the centre of the nucleus and in the outer fringe.

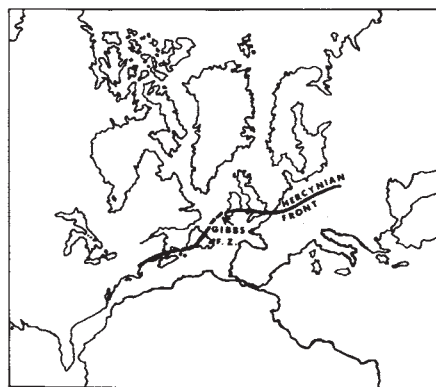
Gibbs Fracture Zone Extends Hercynian Front

THE Gibbs fracture zone, which crosses the present north Atlantic Ocean at about 53°N , is apparently a transform fault which offsets the mid-Atlantic ridge axis by about 350 km in a roughly east-west direction. Westwards from about 35°W longitude to the North American continental margin, the fracture zone has been examined both bathymetrically and using seismic reflection profiling, and an intensive survey has been carried out in the area of the intersection with the mid-Atlantic ridge at about $29^\circ 30'\text{W}$.

To the east of the ridge, on the other hand, there is little available information apart from magnetic anomalies. In *Nature Physical Science* next Monday (October 22), however, Cherkis, Fleming and Massingill correct this deficiency by reporting the results of a survey of the Gibbs zone between 30°W and $16^\circ 50'\text{W}$, and derive some significance from the fact that the zone appears to be in alignment with the Hercynian front in Europe and its extension in North America.

The new seismic profiling and bathymetric data show that the Gibbs fracture zone extends eastwards to at least 17°W , although its characteristics change somewhat as it proceeds. The profile closest to the mid-Atlantic ridge shows that the double-trough feature persists, albeit with reduced definition, and the high central ridge is still prominent. Further east, however, the definition diminishes even further and the central ridge has either disappeared or is hidden by a dense surface reflector; there is now only a single trough, and this lies beneath an acoustically transparent sedimentary mound. Still farther east, the trough widens, sedimentary cover becoming increasingly thick; and in some ways the fracture zone comes to resemble the aseismic fracture zones of the Pacific. The zone is still observable as a buried depression at about 17°W .

The Gibbs fracture zone is well aligned with the structural trends of pre-Jurassic rocks which outcrop on the west European continental shelf and which are believed to represent the most northerly zone of Hercynian orogeny. But the Gibbs zone and the Hercynian front do not link up because the intervening Porcupine ridge and bank probably rotated to their present positions from the European continental block when the north Atlantic at 52°N opened, obliterating the corresponding section of the Gibbs fracture.



Reconstruction by Cherkis *et al.* of the Atlantic before the last spreading phase with positions of the Hercynian Front.

On the other side of the present Atlantic Ocean, the westward extension of the Hercynian front has been traced into North America from the western end of the Gibbs fracture by Hurley (*Sci. Amer.*, **218**, 51; 1968) and others. From the geographical and age relationships involved here, Cherkis and his colleagues conclude that the Gibbs fracture zone existed in the upper Palaeozoic as a front along which crustal plate motion occurred following the opening of the Atlantic. Later, the fracture evolved into the transform fault which now offsets the mid-Atlantic ridge.

ASTRONOMY

IAU General Assembly

from a Correspondent

THE fifteenth General Assembly of the International Astronomical Union was held in Sydney from August 21–30. Associated symposia took place at several centres in Australia both before and after the assembly. These meetings were a fitting tribute to the important contributions to understanding of the southern sky made by Australian optical and radio astronomers.

The invited discourses are usually highlights of the general assemblies and this year's offerings were no exception. C. Townes (University of California, Berkeley) surveyed the rapidly growing field of interstellar molecule research. Twenty-six molecules have now been discovered in interstellar space and include such complex molecules as acetaldehyde (CH_3CHO) and cyanoacetylene (HC_3N). These more complex molecules are of particular interest because they are the starting points for the synthesis of life. Molecules are found in dense clouds mixed with dust which protects them from external dissociating ultraviolet radiation and at the same time probably acts as a catalyst in their production.

D. W. Sciama (University of Oxford) took as the subject for his discourse "The Early Stages of the Universe". He sought to derive the properties of the early Universe from the simplest and most generally accepted observational parameters—namely, its expansion and the isotropy of the microwave background (assumed to be blackbody). The background was thermalised by interactions with matter when the Universe was 10^5 yr old and its density was $\sim 10^{15}$ times greater than at present. If the helium in the Universe is produced cosmically this would occur at an age of 100 s when the density was 3×10^{25} times greater than at present and the temperature was about 10^9 K. In order to go further back in time cosmologists must use general relativity which tells them that the Universe must have had a singularity at some earlier time (or times). Sciama felt that this picture of the early stages of the Universe posed interesting problems which at present are quite open. One is what is the ultimate origin of the blackbody radiation and why does it contain 10^8 – 10^9 photons for every proton?

Great interest was shown in the reports of scientific programmes of the Mariner 9 mission to Mars given by C. Sagan (Cornell University) and H. Masursky (University of Colorado). The photographs (more than 7,000 in all) of Mars from this mission show remarkable detail and reveal a whole range of new features and further, since the photography continued for 12 months,

changes in surface markings could be monitored. One of the oldest controversies about Mars has been the question of the origin of the seasonal and secular variations of surface albedo. Were they the result of biology or of windblown dust? The global pattern of streaks and splotches has now been investigated and is used as an indicator of wind direction and speed. The evidence argues strongly for the action of a wind circulation which must approach wind speeds of half the velocity of sound (~ 150 km h^{-1}) in the 6 mbar atmosphere on Mars. Winds also blow down off the polar caps and possibly even the side of Nix Olympica, the largest volcano in the Solar System, which is 30 km high and 500 km across at the base. These winds move small particles of diameter 0.01 to 0.5 cm about on the surface and are believed to produce the observed changes of albedo. As well as wind erosion there is now unmistakable evidence for water erosion in many areas of Mars. The sinuous tributaries of the 5,000 km long equatorial canyon which are downwards flowing are a prime example. Four channel-forming episodes can be identified going back over more than 10^9 yr. Where is the water now? Sagan thinks it could be locked away in the polar ice caps.

The argument about whether redshifts are cosmological (that they are doppler shifts according to the orthodox view) or not still continues. H. C. Arp (Hale Observatories) took the unorthodox view, arguing that a considerable number of associations can be found between quasistellar objects with high redshifts and galaxies with low redshifts. He claims that in such cases both objects are at the same distance and that the QSO redshift is anomalous and must be due to some physical process other than a doppler shift. Equally he interprets the recent discovery of two QSOs 5" apart in the sky but with very different redshifts as a physical association. This fascinating observation by E. J. Wampler and colleagues (Lick Observatory) shows the two QSOs (one is a 17 mag object with $Z=0.435$ and

the other 19^m with $Z=1.901$) in the position of the radio source 4C11.50 which is also double with a similar separation. In supporting the orthodox view W. Sargent (Hale Observatories) felt that the phenomena discussed by Arp were chance associations; he produced statistical arguments to support this view. To invoke new physics to explain phenomena which were not wildly unlikely in terms of known physics seemed unreasonable. In the case of the QSOs, which on the unorthodox view would contain the greatest non-cosmological component of redshift, there is mounting evidence for them to be at cosmological distances—the continuity of ordinary galaxy, N-type galaxy and QSO properties; the association of QSOs with adjacent galaxies having similar redshifts.

One of the most interesting new observations reported at the meetings was the mapping of the Magellanic Cloud streamers by D. S. Mathewson and colleagues (Australian National University and Radiophysics Division, CSIRO). The main stream seen only in HI so far begins near the Magellanic Clouds, passes through the South Galactic Pole, then extends almost to the galactic plane on the far side of the Galaxy near $l=90^\circ$. It contains filamentary structure elongated parallel to the line of the stream. Additional features are seen on the opposite side of the Magellanic Clouds. These observations are taken to indicate that there was a close passage of the Magellanic Clouds to the Galaxy in the recent past (2 to 5×10^8 yr ago) in which by gravitational tidal action streams of gas were pulled from the Magellanic Clouds in the manner discussed by Toomre and others. By the inverse of the same tidal process the Magellanic Clouds produced the tilt in the plane of the outer part of our Galaxy. But the effect on the Magellanic Clouds has been catastrophic—the material is torn from the clouds and is now believed to be falling into the Galaxy. Interesting new theoretical and observational work will clearly be generated by this discovery.

Radio Emission from P Cygni

SEVERAL P Cygni type stars have recently been identified as radio sources. The discovery that P Cygni itself is also a radio emitter is therefore particularly pleasing, since it would be embarrassing to have an archetype which did not possess one of the most important properties of its class.

In an article in *Nature Physical Science* next Monday (October 22) Wendker and colleagues describe observations of P Cygni at 5 GHz, using the Westerbork Synthesis Radio Telescope and at 10.68 GHz using the 100-m instrument at Effelsberg.

The source does not seem to be variable, and its mean flux density is given by

$$S_5 = 9 \pm 2 \text{ m.f.u.} \\ S_{10.68} = 15 \pm 3 \text{ m.f.u.}$$

The angular size of the radio emitting region is "certainly less than 2 arc s and probably less than 1.5 arc s". P Cygni is a variable which last experienced an outburst 300 yr ago, and Wendker *et al.* are able to reconcile their observations with a model which suggests that emission is seen from the material which the star has lost since that outburst, at present running at some $10^{-4} M_\odot \text{ yr}^{-1}$.

Multiple Polymorphism in Relation to Histocompatibility Antigens

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The extreme polymorphism of the histocompatibility antigens may have evolved to ensure that the body of an individual is not invaded by cells from another individual of the same species.

THE consensus of opinion on the biological significance of histocompatibility antigens, reached in a "workshop" attended by twenty of the most distinguished workers in that field, has been published recently¹. It provides a comprehensive review of the facts, emphasizes the findings that the genetic loci responsible for major histocompatibility antigens in mice lie on either side of a locus (*Ir* 1) concerned with immune response to some synthetic antigens, and notes that certain oncogenic viruses flourish only in mice of certain histocompatibility types. Yet it provides no comments as to the significance of the outstanding feature of all the information about histocompatibility antigens (HCA) that has been accumulated and analysed. This is the intense degree of polymorphism, far exceeding that of any other gene product in vertebrates except the immunoglobulins.

Certainly in man, mouse, guinea pig and chicken and probably also in the rest of the vertebrates, there is a set of very many antigens, based on multiple allelism, at two fairly closely linked loci. The antigens are probably glycoproteins the specificity of which is ascribed to their polypeptide components, which seem to form a single chain with two disulphide bridges, of molecular weight 31,000. The glycoprotein molecules are embedded in the double lipid layer of the cell surface membrane and are present in all nucleated cells, though it is convenient to study them on lymphoid cells. The molecules are mobile within the surface, and if they are removed by enzyme action, the cell soon replaces them.

Most of the experimental work has been done with pure strains of mice, developed in the laboratory and many of them derived from closely related ancestral stocks. Wild *Mus musculus*, however, show just as complex antigenic phenotypes, with the same sorts of "public" and "private" antigens, as are seen in laboratory mice. The detailed specificity of "public" antigens corresponds to some found in laboratory strains, but the "private" antigens are quite distinct. These are quite numerous and any given specificity tends to be found only in mice from some limited locality. Although it is not emphasized in the review, the findings suggest very strongly that new antigens appear by mutation with relatively high frequency and that the possession of any particular antigen confers no selective advantage.

In man, the major histocompatibility antigens of the HL-A series are at least as polymorphic as those in the mouse. There are two distinct gene complexes, LA and Four, to which at least eleven and seventeen different antigens,

respectively, are related. A strong indication of the complexities still to be clarified is given by studying individuals of identical HL-A phenotype, that is, with exactly the same pattern for the four possible antigens in each case. It is found that if the two individuals in question are siblings they will show no blast transformation in mixed lymphocytic cultures and they will accept, or only very slowly reject, cross skin grafts. On the other hand, if two individuals of identical HL-A phenotype are unrelated, the rule is that there will be blast transformation in mixed lymphocytic cultures and skin grafts will be rejected, though neither type of reaction will be as intense as with pairs having no HL-A antigens in common.

Discussion¹ of the significance of these multiple polymorphisms is virtually limited to attempts to find some way by which each different antigen has, or had at some previous phase of evolution, given a positive survival advantage to its carrier. Amos *et al.*¹ admit that there is nothing to suggest that heterozygote advantage, comparable in any way to the sickle-cell heterozygotes, can be postulated and that any influence on the survival of the foetus *in utero* would be in the wrong direction. No attempt was made to look at the possible virtues for survival of multiple polymorphism as such; and, as a necessary corollary, neither Jerne's² nor my own discussions^{3-5,7} of the problem were referred to.

Almost coincidentally with this review¹, one of its contributors, Bodmer⁶, published a generally similar discussion, mainly about the human material, on the evolutionary significance of the HL-A system. There is some overlap, but Bodmer's article includes more extensive discussion of possible interpretations of the polymorphism observed.

Jerne's theory of the interaction of HL-A antigens and the germ-line genes concerned with coding for immunoglobulins is discussed in some detail and my own approach more peripherally. Bodmer's final statement is compatible with an approach I used in 1971 (ref. 7). He suggests that histocompatibility antigen polymorphism, like the immune system, may have been an evolutionary byproduct of the need to develop a cell-to-cell recognition for the differentiation and morphogenesis of multicellular organisms. Accepting this, however, still leaves some serious difficulties that are not overcome by modifying Jerne's hypothesis, so that the antibody patterns inherited in the germ line correspond to hypothetical differentiation antigens, from which the histocompatibility antigens are derived.

Significance of Polymorphisms

The best starting point for reassessing the significance of these polymorphisms is probably the simple statement that in all vertebrates which have been adequately studied there are two types of functional protein showing an abnormally high degree of polymorphism. These are immunoglobulins and the major histocompatibility antigens. Other polymorphisms are common, but the alternative forms rarely number more than three or four, and despite much investigation the only evolutionary virtue in an ordinary poly-

morphism that has yet been demonstrated is that certain red cells, heterozygous for sickle-cell or thalassaemia genes, appear to be less suitable for the multiplication of malarial parasites than normal red cells.

It is now accepted that the immensely complex polymorphism shown by the immunoglobulins is itself an evolutionary adaptation for the specific purpose of preserving the chemical integrity of the body, including means for countering the entry of microorganisms and other types of foreign chemical pattern. (Polymorphism is used here not only in regard to allotype differences but also for the whole range of differences among immunoglobulin molecules both within and between individuals of the species.) The need for multiple polymorphism was evidently so urgent that secondary types of genetic diversity had to be generated in somatic cells as well as at the germ cell level. There is no need to touch on the relative parts played in the generation of diversity by germ line and somatic cell processes or the nature of the somatic diversification process. The essential point is that the diversification is important in itself and special genetic mechanisms have been developed in the course of vertebrate evolution to provide it.

The essence of HCA polymorphism is that it too has all the marks of being evolutionarily important, not for the limited or transient virtues of any individual antigen, but because there is a wide diversity of antigens within the species. It is likely that in all species of vertebrate there are so many distinguishable histocompatibility antigens that they can serve to differentiate every individual in a natural population of the species from any other. The evidence for this virtue of polymorphism in itself can be summarized as follows. First, in all species studied in detail there are more than twenty definable antigenic specificities, and evidence of many others could be demonstrated with more refined techniques and greater numbers of subjects. Second, direct experiment in laboratory mice, the historical experience in breeding the standard pure lines of mice, and the heterogeneity of H2 antigens in wild mice, all point to the conclusion that it is easy for new antigenic phenotypes to arise. This in turn indicates that a genetic mechanism to allow this continuing emergence of diversity has been evolved and by implication is present in all vertebrates. Third, the nature of the major antigens (glycoproteins in which the antigenic specificity is confined to the polypeptide chain) and their situation in the plasma membrane are common to all species that have been examined. Fourth, genetic analysis indicates in each instance the presence of two closely linked gene complexes which are responsible for the major histocompatibility antigens in man and mouse. The system of antigens is not a casual assemblage.

Any search for the biological significance of histocompatibility antigens must start with the firm postulate that their biological significance lies in their diversity and multiplicity and that means exist to ensure that such diversity is maintained. In seeking a reason why diversity of histocompatibility antigens should exist between virtually every individual of a vertebrate species, we can make two preliminary points. First, in view of Bodmer's suggestion that the diversity of histocompatibility antigens has been evolved from cell-to-cell recognition factors concerned in morphogenesis and structural homeostasis, it is important to emphasize that the differences do not concern different cells in the same individual. They concern differences between all the cells of one individual and the cells of any other individual of the same species. Second, immunologically based hypotheses could take various forms, but, broadly speaking, they can be divided into two. Jerne² postulates that the histocompatibility antigens provide a means whereby selection pressures can be developed that will help to generate the diversity of clonal populations of immunocytes that will be needed by the individual.

I, on the other hand, postulate that histocompatibility antigen diversity has been evolved in relation to the immune system to ensure that the integrity of the body is not violated by the invasion of cells from another individual of the same species.

In general terms, Jerne's theory is that the antibody patterns (that is, the amino acid sequences of variable regions) which are coded for by genes transmitted through the germ line are such as will react with any of the histocompatibility antigens that can be found in the species in question. A "starting set" of patterns for antibody is thus provided on which mutation and selection within the developing embryo can build up the diversity of antibodies that the young animal needs to possess when he faces a non-sterile environment at birth. The hypothesis is based on two experimental findings: that the capacity to respond to certain synthetic antigens is linked with certain H2 antigens, and that there is an unexpectedly high capacity for T immunocytes to react with allo-antigens. There has been no detailed interpretation of how these findings are accounted for by the theory nor has it stimulated the disclosure of any hitherto unsuspected observations. My own most serious objection to the Jerne theory is the absence of any indication how a given animal can possess information by which a complementary three-dimensional pattern can be coded for against a chemical configuration (a histocompatibility antigen of a different individual) which is never within the body and may not even have ever existed in the ancestors of the individual we are considering. Bodmer expresses what I think is the same basic difficulty by saying that Jerne's hypothesis "seems to require strict parallel evolution at the population level of histocompatibility antigen genes and immunoglobulin ν genes".

The second approach, for which I am mainly responsible, was first used in the argument that if there were no diversity of histocompatibility antigens and no corresponding arrays of immunoglobulin receptors to react with them, cancer of one animal would be contagious for others of its species, especially for the young⁶. Given the existence of malignant disease as we know it, I can see no rebuttal of the argument at the evolutionary level. It is not easy, however, as Bodmer has pointed out, to see how the solution of this particular problem would require that the population should continue to go on developing new antigen patterns. One may, however, look at the matter from almost the opposite point of view that, because a given antigenic pattern has neither positive nor negative survival value as such, any change by mutation is just as likely to persist as any other allele in the population. On this view, the important genetic change in the first place was to evolve a cell membrane protein of such a character as could allow a wide diversity of amino acid substitutions without any modification of its physiological function. The differences are only significant at an immunological level.

Subsequently, I have been interested in exploring two other possibilities by which foreign cells from another individual of the same species might invade the tissues. The first³ is with viviparous reproduction, especially in forms without a well developed placenta, including ascidians and some fishes. Here the embryo is a different individual, with at least one foreign histocompatibility antigen. The second suggestion⁷ was the speculation that, amongst the earliest free-swimming vertebrates, parasitism of larger individuals by smaller ones of the same or a related species might induce a potentially catastrophic evolutionary situation which called for a means of differentiating self from not-self. It is immaterial which of the three situations was the trigger for the development. They are all basically similar and all present lethal possibilities. All require that any individual can almost certainly differentiate its own tissues from any other individual of the species. To be

sure that less than 1% of chance contacts were compatible, and allowing for incompletely random mixing, would need some twenty distinct antigens, a number which is probably of the order of magnitude that actually exists in most species.

It can also be emphasized here that, however sympathetic one can be to the existence of cell-to-cell mutual recognition proteins concerned with differentiation and structural homeostasis, such functions must have been as universal and important in invertebrates as for vertebrates. Immunological function in the conventional sense is confined to vertebrates. Yet invertebrates differentiate according to genetic programme, repair and regenerate tissues within limits, and deal effectively with potentially invasive micro-organisms. I have always put stress on the need to ask for some specifically new requirement that came to the fore when the cyclostomes evolved. It may have been one of the key evolutionary discoveries when some proto-vertebrate found it could swing some cell membrane protein into a molecule which, unlike its prototype, could accept many mutational changes in part of its structure with no physiological change but with the emergence of potentialities for immunological reaction with an infinity of steric patterns.

The conventional disadvantage of all these modifications of the two hypotheses (Jerne, Burnet) is that they are based on evolutionary occurrences and hence are not subject to experimental verification or disproof. The same objection holds, of course, for the theory of evolution itself and for an enormous range of acceptable topics in the biological sciences. I believe that to eliminate the validity of evolutionarily based interpretations of biological phenomena is thoroughly unwise, particularly when it is clear that comparative studies of cell recognition and immunological phenomena in a full range of organisms will represent a very active field of research in the near future. Comparative studies which are not made meaningful by the use of evolutionary hypotheses are bound to be sterile.

¹ Amos, D. B., *et al.*, *Fedn Proc.*, **31**, 1087 (1972).

² Jerne, N. K., *Eur. J. Immunol.*, **1**, 1 (1971).

³ Burnet, F. M., *Cellular Immunology* (Cambridge University and Melbourne University Presses, 1969).

⁴ Burnet, F. M., *Br. med. Bull.*, **20**, 154 (1964).

⁵ Burnet, F. M., *Acta path. microbiol. scand.*, **76**, 1 (1969).

⁶ Bodmer, W. F., *Nature*, **237**, 139 (1972).

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Does the Mantle Roll?

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Comparison of plume traces with palaeomagnetic data from lithospheric plates for the past 50 m.y. suggests that the mantle has rolled about its own independent axis within an outer lithospheric shell which as a whole is fixed in respect to the Earth's spin axis.

WITH wide acceptance of continental drift as it is now conceived (the independent motion of individual lithospheric plates), consideration of the possibility of true polar wandering seems to have languished. For many Earth scientists polar wandering is associated with palaeomagnetism and is now assumed to be totally unnecessary in order to explain palaeomagnetic data. Divergence of the palaeomagnetic pole from the geographic pole (which is the present situation) is presumed adequate to explain any anomalies.

True polar wandering, *sensu stricto*, refers to a change in the position of the Earth as a whole or as outer lithosphere-mantle shell with respect to its rotation axis, which is presumed to be fixed in space. Such a change could conceivably result from redistributions of mass within the Earth (either by upward convection or downward subduction) whereby its moment of inertia axes shift¹. Should such a change occur, it would have palaeoclimatic impact and would represent a component of

plate motion with respect to the geographic pole, which is common to all plates. Such true polar wandering has no *a priori* connexion with palaeomagnetism unless, as is conventionally assumed, well determined palaeomagnetic poles coincide with the geographic pole.

Whether or not polar wandering has occurred is clearly a question of considerable geophysical significance. Pending the recognition of a frame of reference other than the rotation axis, and identification of a sufficiently precise measure of its position other than by palaeomagnetic studies, the assumption of a coaxial dipole model for the Earth's field must be made if evidence of true polar wandering is sought.

Testing

McElhinny², following McKenzie³, has recently presented a method for testing for true polar wandering, using palaeomagnetic data only but requiring that such data be available for every lithospheric plate. The movement vectors (the apparent polar wander curves) for each plate, weighted by the area of the plate, are summed. By this means the relative motions between plates are cancelled and the resultant vector signifies the magnitude and direction of true polar wandering (that is, a motion common to all plates). As McElhinny has shown, the resultant vector for the past 50 m.y. has a magnitude 2.1°, which he concluded was not significantly different from zero.

McElhinny's method indicates no net movement of the rotation axis with respect to the lithosphere in the past 50 m.y. Duncan *et al.*⁴, however, have shown that, under the assumption that plumes are fixed with respect to the Earth's spin axis, European plate motion predicted by plume traces is not in accord with palaeomagnetic data for the past 50 m.y. They invoked polar wandering to account for the discrepancy. If this discrepancy is due to true polar wandering, then evidence of it

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should be found in the comparison of plume traces and palaeomagnetic data for every plate. We have accordingly explored this possibility. Table 1 presents the result of comparisons of eight plume traces with 50 m.y. polar wander curves for appropriate plates. The 50 m.y. palaeomagnetic poles are essentially those of McElhinny² and the plume trace data are described in the appendix.

The 50 m.y. latitude of the plume, as determined by the great circle distance between the 50 m.y. site of volcanism along a given plume trace and the appropriate 50 m.y. palaeomagnetic pole, should be identical to the latitude of the present site of plume activity if the plume has remained fixed with respect to the Earth's spin axis (=the magnetic pole). A discrepancy implies that the plume and/or the magnetic pole has moved. Assuming the validity of McElhinny's result—that there has been negligible polar wandering during the past 50 m.y.—then discrepancies can be explained only by movement of the plumes with respect to the Earth's spin axis.

The discrepancies calculated in Table 1 are of variable magnitude and some, at least, could be due to error in the location of the plume sites and appropriate palaeomagnetic poles 50 m.y. ago. The palaeomagnetic data are indeed few and of variable quality; they are, however, the same data used by McElhinny and constitute the best that are presently available.

Precise location of even present day "plumes" (assuming they exist) may be difficult; location of 50 m.y. sites poses additional problems. Uncertainty about present day location derives from (a) the long duration of igneous activity at individual sites (for example, 18 m.y. volcanics in the Tristan da Cunha group⁵) and (b) the wide extent of contemporaneous igneous activity associated with individual plumes (for example, contemporaneous, although petrographically distinct, activity along 300 km of the Hawaiian chain⁶).

These uncertainties are compounded by inadequate exposures and age control in identifying 50 m.y. plume sites. Where interpolation or extrapolation was necessary, a uniform spreading rate has been assumed (see appendix). Although we cannot make a quantitative estimate, we feel that for some individual plumes such errors and uncertainties may perhaps cause discrepancies of as much as 10°, and yet we know of no reason why such an error should not be random from one plume to another. Nevertheless, some discrepancies are judged to be too great to arise from such errors, and for the purposes of our analysis all have been treated at face value. Two possible extreme interpretations are: (1) that the plume trace/palaeomagnetic discrepancies are due to random movement of plumes in time; the population of plumes should not, then, maintain a stable configuration; (2) that plumes are fixed with respect to one another but move in unison as the mantle rolls about some axis independent of the Earth's spin axis. Discrepancies between the plume traces and the palaeomagnetic polar wander paths would be expected, their magnitude varying as the distance of given plumes from such a "mantle roll" axis. A stable configuration of plumes, however, is preserved.

Morgan⁷ concluded that interplume motion in the Pacific, at least, could have amounted to as much as 1 cm yr⁻¹. Minster

*et al.*⁸ conclude that their instantaneous plate motion model is compatible with the hypothesis that, with the conspicuous exception of Iceland, all other plumes are stable with respect to one another.

Discrepancies

To test whether the discrepancies in Table 1 might be due primarily to random interplume motion, or to "mantle roll", the following method was used. If there is a "mantle roll" axis about which the mantle has rotated during the past 50 m.y., it should be identified by a least-squares fit of the data from Table 1. That is, the latitudes of the present plume sites can be changed, by rotation about an axis, to those 50 m.y. latitudes inferred from the palaeomagnetic evidence. Using an iterative program, one axis from a grid of mantle roll axes is chosen and the best fitting angle of rotation is found by the method of non-linear least squares⁹. The success of the fit is measured by the sum of squares of residuals, which should be minimized at the best fitting axis. The sum of squares of residuals (SSR) = $\sum \theta_i - f_i(\theta_p, \phi_p, \alpha)^2$ where θ_i are the 50 m.y. latitudes and $f_i(\theta_p, \phi_p, \alpha) = \hat{\theta}_i$ are the latitudes of the rotated present plume sites. θ_p and ϕ_p are the latitude and longitude of the chosen axis and α is the rotation angle. In this calculation θ_i and $\hat{\theta}_i$ are expressed in rad. This process can be continued over a grid of trial axes covering a hemisphere and the resulting array contoured to determine the best fitting mantle roll axis and angle of rotation.

The result of such a test using the data in Table 1 is illustrated in Fig. 1. The specific best fit axis is at 25°N, 30°E, with an angle of rotation of 12.1°, which decreases the SSR from 13.1 to 3.1. It is more realistic to recognize a sigmoidal "region" of good fitting axes between 20° and 40°E. This meridional region of good fitting axes about the best fitting axis is, however, well defined and distinct from the poorer fitting axes over the rest of the hemisphere (Fig. 1). The data seem to be insufficient to impose a clear latitudinal constraint (in terms of residuals) on the location of the best fit axis; but the angular rotations called for increase prodigiously for axes at the extreme latitudes.

Iceland accounts for the largest latitude discrepancy in Table 1; it is also the conspicuous exception to the stable plume model of Minster *et al.*⁸. To ascertain whether this discrepancy is the primary cause of the mantle roll calculated, we repeated the test omitting the Iceland data altogether. The longitudinally constrained (20°–40°E) sigmoidal region of best-fit mantle roll axes is again obtained, with a comparable improvement by a factor of 4 in the SSR (from 6.5 to 1.5). The specific best fit axis is now located at 45°N, 30°E, with a clockwise rotation of 12°.

In general, these results mean that 12° clockwise rotation of the mantle about an axis emerging in the northern Sudan greatly improves the fit of 50 m.y. plume-site latitudes inferred from palaeomagnetic evidence to the latitudes of their present sites (see Table 2). A still better fit may be achieved by allowing interplume movement but the close fit under the requirement of whole-mantle roll suggests a high degree of common motion.

Table 1 Palaeomagnetic and Plume Data

Plume	Present site		50 m.y. site		50 m.y. magnetic pole		α_{95}	50 m.y. latitude	Discrepancy
	Latitude	Longitude	Latitude	Longitude	Latitude	Longitude			
(1) Iceland	64.0°N	342.0°E	62.0°N	353.0°E	77.0°N	161.0°E*	2.0	47.2°N	14.8°N
(2) Yellowstone	45.0°N	250.0°E	38.0°N	238.0°E	85.0°N	197.0°E	14.0	41.7°N	3.3°N
(3) Tibesti	20.0°N	19.0°E	32.0°N	13.0°E	81.0°N	168.0°E	4.5	23.8°N	3.8°S
(4) Hawaii	19.0°N	204.0°E	37.0°N	171.0°E	71.0°N	354.0°E	13.4	18.0°N	1.0°N
(5) Balleny	67.0°S	165.0°E	42.0°S	145.0°E	70.0°N	306.0°E	5.3	60.4°S	6.7°S
(6) Réunion	21.0°S	55.0°E	5.0°S	70.0°E	70.0°N	306.0°E	5.3	15.8°S	5.2°S
(7) Tristan (E)	37.0°S	347.0°E	37.0°S	363.0°E	81.0°N	168.0°E	4.5	45.7°S	8.7°N
(8) Tristan (W)	37.0°S	347.0°E	34.0°S	333.0°E	79.0°N	122.0°E†	14.0	43.2°S	6.2°N

* This is the pole obtained directly from the Færoes lavas²²; McElhinny² used 75°N, instead of 77°N, in his analysis.

† McElhinny² does not give a separate 50 m.y. magnetic pole for South America. This Tertiary pole is from Creer²⁴.

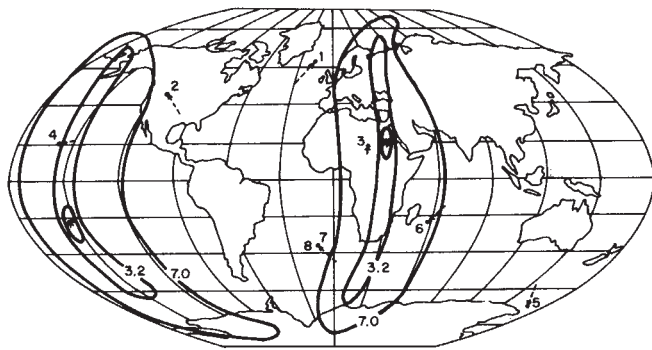


Fig. 1 Contours on surface of "sum of squares of residuals" (SSR, see text) to show location of best fit rotation axis, and the pattern of increase in SSR away from it. Clockwise rotation of the mantle of 12.1° about an axis at 25°N , 30°E decreases the SSR from 13.1 to 3.1. Dashed lines with arrows indicate the motion of plumes (as numbered in Table 1) predicted by this mantle roll.

By the same argument the result, we feel, is inconsistent with the discrepancies of Table 1 being due entirely to random error in palaeopole or plume location.

This leads to the curious picture of the lithosphere essentially fixed to the Earth's spin axis with the mantle beneath rolling about some independent axis. Roll of the mantle, uncoupled from the lithosphere, is compatible both with McElhinny's conclusion that polar wander with respect to the lithosphere has not occurred within the past 50 m.y., and with the variable discrepancies between plume traces and polar wander paths. Grommé and Vine¹⁰ conclude that the Hawaiian plume has not changed its position significantly with respect to the magnetic pole since the formation of Midway Island (18 m.y. ago), whereas Duncan *et al.*⁴ proposed that the Iceland and Eifel plumes have moved considerably. This conflict is possibly resolved by the fact that Hawaii is close to the proposed mantle roll axis (the antipode at 25°S , 150°W) and would not be expected to move a great distance (in fact the plume motion predicted is primarily westwards, rather than translatitudinal), whereas Iceland is more than 60° away and should then show substantial movement. The possible causes or consequences of this hypothetical process of mantle roll will not be considered here. We feel, however, that the evidence for it is suggestive and encourages its consideration and the acquisition of data in sufficient quantity and quality for a more decisive test.

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Appendix

The following are the geochronological data for plume traces.

- (1) Iceland Plume: the Wyville-Thompson Ridge connects the present site of plume activity to the Faeroe Islands, aged 50 m.y.¹¹.

Table 2 Latitude Discrepancies Observed and Predicted by 12.1° Clockwise Mantle Roll about an Axis at 25°N , 30°E

Plume	Latitude discrepancy	
	Observed	Predicted
Iceland	14.8°N	8.6°N
Yellowstone	3.3°N	7.9°N
Tibesti	3.8°S	2.0°N
Hawaii	1.0°N	0.3°S
Balleny	6.7°S	8.4°S
Réunion	5.2°S	5.3°S
Tristan (E)	8.7°N	6.6°N
Tristan (W)	6.2°N	6.6°N

The N or S notation signifies that with respect to the Earth's rotation axis, the present plume site is (or predicted to be, with mantle roll) north or south of its position 50 m.y. ago.

- (2) Yellowstone Plume: the trace to the Snake River plains, 25 m.y. ago, is conspicuous (R. L. Armstrong, W. P. Lee-man and H. E. Malde, to be published) but its earlier manifestations (nearer the continental edge) are obscure. Extrapolation along the Yellowstone-Snake River trace at a velocity of 2 cm yr^{-1} would locate its 50 m.y. position at San Francisco; this palaeoposition has been used.
- (3) Tibesti Plume: a conspicuous line of igneous centres curves generally north-west from Tibesti (very recent activity) toward Tripoli¹². Although volcanism along this line has recurred up to the Pliocene (at least), the oldest activity near Tripoli has been dated at 52 m.y. (ref. 13). This has been taken as the palaeopluve site.
- (4) Hawaiian Plume: present activity at Hawaii is connected by a trend of volcanism to the Emperor Seamounts^{6,7,14-16}. A 50 m.y. position has been interpolated from known ages and inferred plate velocity¹⁷.
- (5) Balleny Plume: the presently active Balleny Islands sit at the south end of the South Tasman Rise, a conspicuous aseismic ridge connecting the Australian-Antarctic spreading ridge to Tasmania. Although the well known Tasmanian dolerites are Jurassic in age, seafloor anomalies indicate initiation of the ridge adjacent to Tasmania at about 50 m.y.¹⁸. The South Tasman Rise also parallels the Tasmanid Guyots¹⁹ and eastern Australian volcanism.
- (6) Réunion Plume: the Réunion-Laccadive-Maldives line⁷ is interrupted by a recent spreading episode. The south end of the Chagos Plateau is proposed as the 50 m.y. site of volcanism based on magnetic anomaly evidence^{20,21}.
- (7) Tristan (East) Plume: volcanism at Tristan da Cunha near the mid-Atlantic Ridge is linked by the Walvis Ridge to South-west Africa. Again, the intersection of anomaly 20 (~ 50 m.y.) with this aseismic ridge gives the 50 m.y. site of plume activity^{21,22}.
- (8) Tristan (West) Plume: towards South America the plume appears as the Rio Grande Rise. Anomaly 20 marks the 50 m.y. site^{21,22}.

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Chemical Oscillations in a Membrane

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An oscillation was observed in a computer simulation of a single first-order reaction in a membrane exhibiting both the chemodiffusional coupling and autocatalysis.

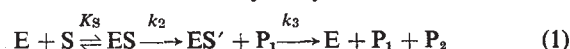
THERE has been a recent upsurge of interest in the occurrence of chemical oscillations, prompted largely by their probable biological importance (see refs 1–5 for review). The earliest recognition that sustained periodic behaviour may be exhibited by open chemically reacting systems incorporating an autocatalytic step came from the studies of Lotka^{6–8}. Katchalsky and Spangler predicted that oscillations might be achieved in certain circumstances by coupling a reaction to a membrane transport process⁹; suggested conditions include a catalytic system undergoing a phase transition with metastable states induced by reactant or product. We shall show that it is possible to obtain oscillations in numerical simulations of a similar system with more realistic and less restrictive conditions.

Periodicity has been found in a number of simulation studies based on biochemical models, all representing homogeneous systems far from equilibrium and characterized by more or less complex kinetic schemes. Such systems generally show limit cycle behaviour, that is, oscillation proceeds around an unstable steady state¹⁰. This was illustrated in an instructive analysis of a hypothetical reaction scheme involving a trimolecular autocatalytic step, which established the emergence of both temporal and spatial order ("dissipative structures") given suitable conditions^{11–13}. A linearization procedure has been proposed to evaluate the hitherto undetermined effects of diffusion on autocatalysis in a heterogeneous system¹⁴.

Very few well defined experimental examples of sustained chemical oscillations exist. The two which have attracted most attention are the Belousov-Zhabotinski malonic acid oxidation reaction^{15,16}, which readily forms spatial patterns¹⁷, and the phosphofructokinase reaction of the glycolytic pathway³. Both have been studied extensively in homogeneous solution, the latter in cell-free extracts or reconstructed systems¹⁸. The Belousov-Zhabotinski reaction has also been carried out in a membrane containing immobilized catalyst (ferroin), thus eliminating convection¹⁹. In both instances elaborate kinetic schemes have been proposed to account for the observed behaviour^{18,20,21}.

In an attempt to see whether chemical oscillations can be generated by relatively unsophisticated means, a simple and well characterized system exhibiting both chemodiffusional coupling and autocatalysis was chosen for computer simulation: the hydrolysis of benzoyl-L-arginine ethyl ester (BAEE) by a membrane containing bound papain. The collodion-papain membrane has been studied extensively as a prototype enzyme membrane^{22–24}, and the kinetics of BAEE hydrolysis in solution

are well understood²⁵. Both in solution and in the membrane the reaction follows a bell-shaped pH dependence; in solution the maximum rate occurs at neutral pH while in the membrane the overall rate curve, plotted as a function of external pH, appears to be displaced towards the alkaline region as a consequence of the local increase in hydrogen ion concentration^{22,23}. In both cases the alkaline limb of the rate curve represents an autocatalytic effect. Assuming that the membrane is homogeneous and that the reaction kinetics are not altered by virtue of the immobilization of the enzyme (experimental papain membranes may contain over 90% of water), the local reaction scheme is identical to that in solution. Hydrolysis proceeds through the formation of an acyl-enzyme intermediate ES'.



ES is the enzyme-substrate complex, K_S its dissociation constant and P_1 and P_2 are the alcohol and acid moieties of the ester substrate. The pH dependence of the reaction rate can be accurately described by the relations²⁵

$$K_S = 5.45 \times 10^{-8} \text{ mol cm}^{-3} \quad (2)$$

$$k_2 = 64.9 / (1 + 10^{7.29} [H] + 10^{-11.49} [H]^{-1}) \text{ s}^{-1} \quad (3)$$

$$k_3 = 20.2 / (1 + 10^{6.92} [H]) \text{ s}^{-1} \quad (4)$$

where $[H]$ is the hydrogen-ion concentration (mol cm⁻³). The rate, as a function of both substrate and hydrogen ion concentration, is given by

$$R([H], [S]) = k_2 k_3 [E] / (k_2 + k_3 + k_3 K_S [S]^{-1}) \text{ mol cm}^{-3} \text{ s}^{-1} \quad (5)$$

where $[E]$ and $[S]$ are the enzyme and substrate concentrations (mol cm⁻³). This expression is readily cast into the usual Michaelis-Menten form, from which it is seen that the apparent K_M increases from about $K_S/4$ to K_S over the pH range 4 to 10.

In formulating the local equations of continuity for reaction within the membrane, it is assumed that electrical effects can be neglected. This will be the case providing the membrane has an open structure and the ionic strength of the medium is maintained sufficiently high by means of a neutral electrolyte such as KCl. We can then write

$$-R([H], [S]) + D_S \nabla^2 [S] - \partial S / \partial t = 0 \quad (6)$$

where D_S is the diffusion coefficient of the substrate within the membrane. However, for the hydrogen ion

$$R([H], [S]) + D_H \nabla^2 [H] - \partial [H] / \partial t = \partial [H_2O] / \partial t = D_{OH} \nabla^2 [OH] - \partial [OH] / \partial t \quad (7)$$

where we take into account the presence of hydroxyl ion, since in the autocatalytic region its concentration exceeds that of hydrogen ion. Equilibrium between water, hydrogen, and hydroxyl ion may be considered to be established instantaneously ($K_w = [H][OH] = 10^{-14} \text{ mol}^2 \text{ cm}^{-6}$) as compared with the hydrolysis rate. Assuming that the membrane diffusion coefficients D_H and D_{OH} are approximately equal, (7) simplifies to

$$R([H], [S]) + D_H \nabla^2 ([H] - [OH]) - \partial ([H] - [OH]) / \partial t = 0 \quad (8)$$

For simulation studies we adopted the simplest possible

boundary conditions: identical well stirred solutions in reservoirs on either side of the membrane, with fixed concentrations. In this case, since the membrane is homogeneous, it may be divided conceptually into two mirror-image layers with an impermeable plane or wall between them. Dirichlet conditions govern the outer boundaries $[S]=[S]_0$, $[H]=[H]_0$, Neumann conditions the inner ($\partial S/\partial x = \partial[H]/\partial x = 0$). The simulations were performed for one such membrane layer of thickness L_M , corresponding closely to an experimental design adopted later²⁹. Using a standard finite-difference algorithm, preliminary simulations were made with $L_M=100\text{ }\mu\text{m}$, a reservoir pH of 10 ($[H]_0=10^{-10}\text{ M}$), and published values of $[E]$, D_H , and D_S for the papain-collodion membrane²⁹. A sustained oscillation of period 40 s is observed within the substrate concentration range $[S]_0=5\times 10^{-4}\text{ M}\pm 10\%$, and an extremely sharp pH "front" develops, and moves back and forth across the membrane thickness during a large fraction of the oscillation period. At higher substrate levels the oscillation ceases but the front persists.

In subsequent experimental studies, oscillatory behaviour was achieved in cross-linked papain-albumin membranes, and the modulating influence of the unstirred "boundary" layer at the reservoir-membrane boundary was recognized (A. Naparstek, D. Thomas and S. R. Caplan, unpublished). For purposes of comparison with these experiments further simulations were based on the parameters of the papain-albumin membrane, taking into account diffusion across a boundary layer of thickness L_B by assuming, as a first approximation, diffusion coefficients identical with those in the membrane. In a typical example $L_B=26.5\text{ }\mu\text{m}$ and $L_M=11\text{ }\mu\text{m}$ ($[S]=[S]_0$, $[H]=[H]_0$ at $x=0$, $R([H],[S])=0$ for $0\leq x\leq L_B$, $\partial[S]/\partial x=\partial[H]/\partial x=0$ at $x=L_B+L_M$). Figures 1 and 2 illustrate the results obtained for this system with the new, faster, and more accurate split-step Fourier algorithm (N. J. Zabusky and R. Hardin, unpublished). (An analytical/numerical investigation validated this algorithm: an essential feature of the study is a linear stability analysis, and perturbations are taken about the inhomogeneous stationary states.) Figure 1 shows three pH profiles: *a* and *c* are for values of $[S]_0$ that yield stable stationary states whereas *b* is for an intermediate $[S]_0$ when the system is oscillatory with a period of 18.1 s. The profile *b* corresponds to the closest approach of the pH front to the reservoir, as shown by the vertical arrows in Fig. 2 where complete representations of the solutions for $[H](x,t)$ and $[S](x,t)$ are given on space-time contour diagrams. The steep pH front is seen to exist for 7.5 s, 3.4 s of which are

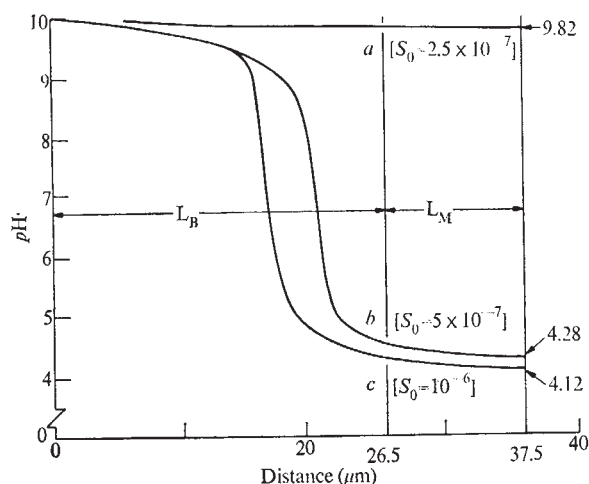


Fig. 1 pH profiles for the model equations of a papain-albumin membrane²⁶ ($[E]=5.23\text{ mol cm}^{-3}$, $D_H=5.5\times 10^{-6}\text{ cm}^2\text{ s}^{-1}$, $D_S=1.2\times 10^{-6}\text{ cm}^2\text{ s}^{-1}$). Reservoir substrate concentrations are $[S]_0=2.5\times 10^{-7}\text{ mol cm}^{-3}$ for *a*; $[S]_0=5\times 10^{-7}\text{ mol cm}^{-3}$ for *b*; $[S]_0=10^{-6}\text{ mol cm}^{-3}$ for *c*. Curves *a* and *c* are stable stationary states; *b* corresponds to the times marked by vertical arrows in Fig. 2. The reservoir pH has been set at 10.0, the boundary layer thickness $L_B=26.5\text{ }\mu\text{m}$, and the membrane thickness $L_M=11.0\text{ }\mu\text{m}$.

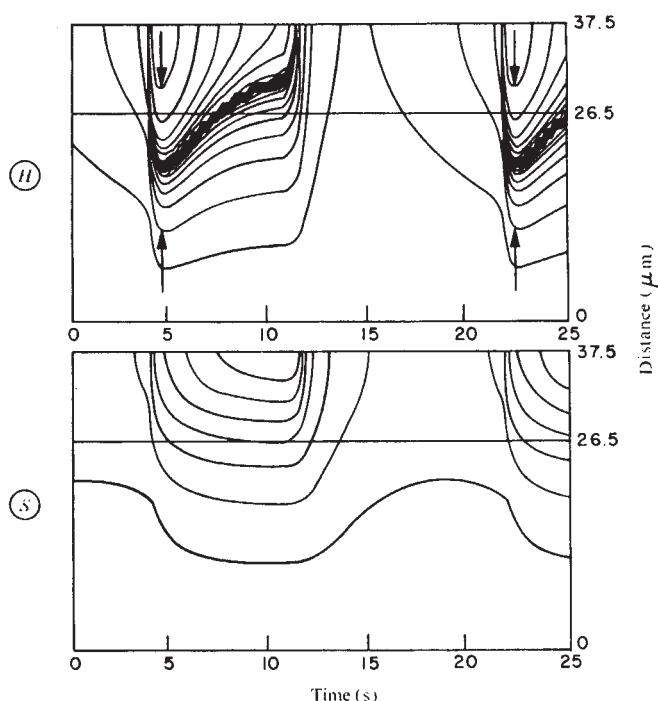


Fig. 2 Space-time $[H]$ and $[S]$ contours (logarithmically spaced) for the model equations of a papain-albumin membrane in the oscillatory regime. The period of the oscillation is 18.1 s and the amplitude at the impermeable plane varies from 9.92 pH units to 4.27 pH units. The reservoir substrate concentration is $5\times 10^{-7}\text{ mol cm}^{-3}$ (see legend to Fig. 1 for additional parameters).

spent in the boundary layer. At any location in the membrane the $[S](x,t)$ against $[H](x,t)$ trajectory appears as a limit cycle. Additional simulations, with L_M increased, show that the front may not reach the impermeable wall, and small amplitude oscillations occur.

For the substrate concentrations considered here, the reaction remains in the first order kinetic range everywhere within the membrane. This is also true of the higher concentration, $4.5\times 10^{-3}\text{ M}$, used in the experimental study. At this substrate level, however, stable stationary states (showing steep transitions) are obtained by simulation. Nevertheless, the close agreement of the periods (20 s in the laboratory and 18.1 s by computation) and the character of the solutions suggest that this model provides a sound basis for understanding the reaction-diffusion dynamics of the system.

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LETTERS TO NATURE

PHYSICAL SCIENCES

Possible Indirect Method for the Detection of γ -Ray Flashes from Supernovae

THE electron-photon cascade calculations of Messel and Crawford¹ contain a remarkable feature which has not attracted widespread attention. Although the secondary electrons reach maximum numbers and die away in a fashion broadly consistent with previous calculations, very low energy γ rays persist in relatively large numbers to great depths of absorber. For a primary photon of 500 MeV in air, for example (Table K756 of ref. 1), and secondary electrons and photons of total energy ≥ 1 MeV, the mean electron number falls to 0.78 at 10 radiation lengths (r.l.), and to 0.009 at 16 r.l.; but the number of photons, reaching a maximum of 11.1 at 5 r.l., is attenuated very slowly, and 5.48 are still predicted at 30 r.l. The total energy carried is 10.46 MeV, so the average predicted energy at 30 r.l. is about 2 MeV. Similarly for higher energies: a 50 GeV primary photon produces 577 γ rays of mean energy 2.04 MeV at 30 r.l. In general, it seems that for primaries of more than 500 MeV, about 2% of the primary energy reaches 30 r.l. in the form of 1 to 2 MeV γ rays; 30 r.l. in air corresponds to about 1,100 g cm⁻², or sea level.

Other cascade calculations suggest considerably smaller numbers of low energy γ rays, though there do not seem to be any precisely comparable results available at these energies and depths in air. It has been appreciated for many years (see, for example, Rossi², page 223) that γ rays of energy lower than about 20 MeV have mean free paths of several radiation lengths before falling below 1 MeV, but the absorption of $\exp\{-0.11t\}$ quoted by Messel and Crawford seems longer than expected previously.

If the Messel and Crawford figures are correct, a burst of high energy γ rays striking the top of the atmosphere may be observable by a γ -ray detector of feasible size and efficiency at sea level; and because of the very slow attenuation, such a system may be more effective at sea level than at mountain or balloon altitudes. Consider a burst of γ rays of total energy 10^{-6} erg cm⁻² above 500 MeV striking the top of the atmosphere in a time τ . This corresponds, with an integral spectrum $E^{-1.5}$, to $\sim 10^{-3}$ γ rays cm². At sea level we have 2×10^{-8} erg cm⁻². At an average energy of 2 MeV, this is 10^{-2} photons cm⁻² or 100 photons m⁻².

With a detector area of 1 m² and an efficiency of 30%, which would seem feasible with a matrix of NaI or CsI crystals, this would give thirty events in the system. The

background counting rate would be of the order of 1,000 counts⁻¹. The detectability of the effect then depends on the duration of the pulse τ . If τ is 1 ms, a continuously clocked reset-scaler overflow system, demanding fifteen pulses in this time, would have a random rate of only about one event in 10^4 d. A real signal rate of one event per year would therefore be detectable.

Estimates of the γ -ray flash from a supernova have been made by Colgate^{3,4}. In his first theory, a supernova in the Virgo cluster would produce 0.1 to 1 γ ray cm⁻², of characteristic energy 2 GeV, with a pulse duration 10^{-5} s, at the top of the atmosphere. This corresponds to 3×10^{-4} to 3×10^{-3} erg cm⁻². At sea level a burst of 3×10^4 to 3×10^5 2-MeV γ rays m⁻² would be produced, which would be very easily detectable, though it could be confused with an extensive air shower if dead times were not introduced to ensure that the events were spread over a time longer than ~ 2 μ s. In a modified theory⁴⁻⁶, however, the expected flux is reduced by a large factor from the original estimate, to about 10^{-7} erg cm⁻² in about 10 ms. Very recently, Colgate (preprint, May 1973) has again given an estimate for ultrahigh energy γ rays, predicting 10^{44} ergs of $\sim 10^{12}$ eV photons from a type I supernova. For a distance of 10 Mpc, this would produce 10^{-8} erg cm⁻² at the top of the atmosphere, which fails in detectability by about two orders of magnitude.

Recently⁷, satellite observations of low energy γ -ray bursts, which may come from supernova explosions, have been reported. Energy fluxes between 0.2 and 1.5 MeV were 10^{-5} to 4×10^{-4} erg cm². Pulse durations up to 30 s were observed, but an initial fast pulse, predicted by the Colgate model, would not, apparently, be excluded. If the observed γ rays are the products of essentially high energy processes, a flux of 10^{-6} erg cm⁻² at ≥ 500 MeV would not seem impossible. Colgate (preprint, *op. cit.*), however, ascribes these γ rays to essentially low energy processes in type II supernovae.

An experiment based on the principle of low energy γ -ray detection at sea level has been used⁸ to search for sporadic γ rays from pulsars, with essentially negative results. The detectors used were, however, liquid and plastic scintillators of low efficiency and area; and isolated events could not have been separated from random coincidences under the conditions of operation. It seems possible that isolated bursts of 100 low energy γ rays m⁻², occurring perhaps ten times a year, could have escaped previous detection in cosmic-ray or other experiments.

The method may be applicable to searches for γ rays from other phenomena besides supernovae. In particular, a limit could be set for pulses associated with the gravitational events reported by Weber⁹. The energy in these is about 10^4 erg cm⁻², so for a flash of duration 1 ms, the limiting intensity of 10^{-6}

erg cm⁻² in γ rays represents a fraction 10^{-10} of the gravitational energy. For a pulse duration of 1 s, about 10^{-4} erg cm⁻² should produce an event. These energy limits are comparable with those set by Baird and Pomerantz¹⁰ at X-ray frequencies. In terms of energy per unit bandwidth, the γ -ray limits would be less than 10^{-30} W m⁻² Hz⁻¹, very much smaller than those set at radio, optical or X-ray frequencies¹¹⁻¹⁵, and are far lower also than those attainable in microwave searches for supernovae¹⁶; but it is doubtful whether the use of this unit is physically significant at γ -ray frequencies. In terms of total energy, the γ -ray limit is about 10^4 times greater than that of ref. 16.

Since the feasibility of the method depends entirely on the Messel and Crawford predictions, an experimental test of the theory would be desirable. This would be difficult in air; but in light solids, or in copper or lead, where similar predictions are made, it would seem relatively easy at accelerator energies. The literature on radiation shielding, although very extensive, does not seem to provide data which can be used for this purpose. The energy spectrum of the predicted γ -ray flux is very steep, numbers of photons at ≥ 7 MeV being only about 0.5% of those at ≥ 1 MeV.

Even if the attenuation of the low energy γ rays is significantly greater than predicted, the method may still be feasible at mountain altitudes, though larger cosmic-ray background will increase detectable fluxes. The general condition for detectability is that if τ is the duration of the signal flash, R the background count rate in the detector, n the number of pulses in a given signal, and S the rate at which signals occur, $S \gg R^n \tau^{n-1}/n!$; and also $R\tau \ll 1$.

The method would appear suitable in practice only for signal durations less than about 10 ms.

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Dynamically Plausible Hypotheses of Lunar Origin

THE necessity that at least half the primordial Moon be enriched in refractory aluminium and calcium silicates^{1,2}, as well as this outer half being depleted in volatiles and the entire Moon being depleted in iron, has led to a renewed emphasis on the capture hypothesis of lunar origin³⁻⁷. This emphasis

in part springs from the need to have a high temperature (or low pressure) environment in which condensation, and hence presumably accretion, of substances less volatile than iron predominates^{8,9}. The evident reasoning is that the Moon was made in a different place than it is now; the philosophy seems to be that celestial mechanical improbabilities are more tolerable than are thermochemical improbabilities, in view of the fewer bodies involved.

All those who hypothesise capture acknowledge that it is an improbable event. So most⁵⁻⁷ suggest that the Moon was made somewhere near the Earth, which seems contrary to the thermochemically inspired wish to make the Moon in a different part of the Solar System. All but one⁴⁻⁷, however, explicitly require the entire Moon to be captured at once, which is the most severely implausible form of the hypothesis. The reason for this implausibility is the extreme weakness of the only known energy sink for pure capture, tidal friction.

It is therefore worthwhile to emphasise more strongly than previously¹⁰ why tidal friction is an implausibly weak energy sink. A way to demonstrate this weakness is to assume that most, but not all, of the Moon was captured; to hypothesise that the energy sink for this capture was collision with matter already in orbit around the Earth; and then to ask how much matter is needed to make such collisions a more effective energy sink than tidal friction. For a given dissipation factor ($1/Q$ and perigee distance r_p), the energy dissipation per pass by tidal friction in the initial highly eccentric orbit is easily estimated, since, as shown by Gerstenkorn¹¹, for plausible rheologies this dissipation is mainly in the radial tides in the Moon. So we can write the dissipation in terms of the work done^{12,13} on a homogeneous Moon in a single approach from a distance to perigee:

$$\begin{aligned} \Delta E &= (1/Q) \oint_{\text{volume}} \rho \dot{u}_i [(U+T)/x_i] / dS dt \\ &= (2/Q) \oint_{r_p}^{\infty} \oint_{\text{surface}} (\rho h/g) \partial U / \partial r (U+T) ds dr \\ &= 2/Q \int_{r_p}^{\infty} 2\pi R^2 \int_0^{\pi} \frac{m}{4\pi R^3/3} \cdot \frac{h}{Gm/R^2} \left[-\frac{3GMR^2}{r^4} P_{20}(0) \right] \\ &\quad \left[\frac{(1+k)GMR^2}{r^3} P_{20}(0) \right] d\theta dr = \left(\frac{3}{10} \right) \frac{h(1+k)R^5}{Qr_p^6} \frac{GM^2}{Qr_p^6} \quad (1) \end{aligned}$$

where h , k are the displacement and potential Love numbers, about 0.033 and 0.020 for the present Moon¹⁴, R is the Moon's radius, G is the gravitational constant, and M is the Earth's mass. Assuming a Q of 10 and a minimum perigee distance r_p of 1.5 Earth radii, equation (1) gives 0.5×10^{33} erg dissipation per pass.

For collision of a Moon of mass m and velocity v with another body of mass m' and velocity v'

$$\Delta E = 1/2 [mv^2 + m'v'^2 - (m+m')v_0^2] \quad (2)$$

$$mv - m'v' = (m+m')v_0 \quad (3)$$

For $v = 10$ km s⁻¹ and $v' = 7$ km s⁻¹, the use of the momentum conservation equation (3) to eliminate v_0 in equation (2) and 0.5×10^{33} erg for ΔE yields 1.4×10^{21} g for m : that is, the Moon needs to collide with only 2×10^{-5} of its own mass for collision to be a more efficient energy dissipator than tidal friction.

Of course, in a single pass the Moon would have collided with only a minor portion of the matter in orbit about the Earth; a more realistic estimate requires a hypothesis as to the distribution of this matter. For a uniform disk of thickness b much less than radius c , an orbit of inclination I appreciably greater than b/c will collide with a volume $2b\pi R^2/\sin I$ per pass, or $2R^2/(c^2 \sin I)$ of the total. For a c of, say, three Earth radii, this ratio is still more than 0.015. So for this plausible model, a total disk mass of only 1/600 the Moon's mass would have been a more effective energy sink than tidal friction.

It is sometimes argued that solar perturbations may keep

the Moon close to the Earth long enough for tidal friction to be effective^{6,7}; however, the effectiveness of collision dissipation will be equally increased by the same mechanism, so that its comparative advantage is maintained. Another mechanism proposed by Alfvén and Arrhenius⁷ is the locking of the Moon in synchronisation with a longitudinal variation in the Earth's gravitational field. This synchronisation cannot be 1:1, since for the primordial day of 5 h the orbit would be within the Roche limit. Higher order synchronisations depend on the orbital inclination I or eccentricity e . The torque exerted by the fixed variation in the gravity field must then be greater than the tidal torque, or^{13,15}:

$$\alpha(GM/r)(R/r)^i J_{lm} e^{-i} I^j \gtrsim (k/Q)(Gm/r)(R/r)^2 (R/r)^3 \quad (4)$$

where R is now the Earth's radius, J_{lm} is the dimensionless spherical harmonic potential coefficient, α is a factor of order unity, and the exponents i, j depend on the order of the commensurability. For example, for a 2:1 commensurability $i+j \geq 2$ for $l=2$ and $i+j \geq 1$ for $l=3$. At a 2:1 commensurability for the 5-h day, r/R is 3.7. Thus

$$J_{22} \gtrsim (k/Q)(m/M)(R/r)^3 (1/ae^2) \approx 0.7 \times 10^{-4} / (Qe^2)$$

$$\text{or } J_{31} \gtrsim (k/Q)(m/M)(R/r) (1/ae) \approx 0.3 \times 10^{-3} / (Qe)$$

There is a marginal possibility the J_{22} commensurability would be stable until radial tides pumped down the eccentricity. This says nothing about the probability of capture into the resonance, which depends on the change in tidal torque with passage through commensurability¹⁶.

Returning to the main theme, a mechanism is needed for the Earth to acquire the 1/600 Moon to catch a whole Moon. In any model of planet formation from a gas and dust nebula, at the stage where protoplanets become sufficiently massive for gravitational capture to be significant, there still will be an abundance of smaller bodies: a hierarchy of planetesimals of number density varying inversely with mass over a wide range of sizes. The attraction of the protoplanet for these smaller bodies will cause an increase in the number density of small bodies in the vicinity of the protoplanet, and thus an increased probability of mutual collisions between small bodies. The calculation of this probability is somewhat intricate; it has been done best by Safronov¹⁷. In any case, given such a collision in the vicinity of a protoplanet, the chance is quite good that the bodies will lose enough energy to be captured by the protoplanet, but retain enough angular momentum to go into orbit around the protoplanet, rather than falling in.

Once any matter was in orbit around a protoplanet, it acted as an effective trap for further infalling matter, thus increasing the mass of the circumplanetary ring. Most of this matter was in the form of planetesimals of radius less than 300 km, the consequence of gravitational instability of condensed matter in the nebula, followed by a period of collisions, as derived by Safronov¹⁷ and later by Goldreich and Ward¹⁸. So the probability is greatest that satellites are created out of many smaller bodies in orbit around the planet: this is essentially the model of Ruskol¹⁹⁻²¹. But the anomalies in planetary rotation, Venus and Uranus, the larger mean eccentricities and inclinations of the small planet orbits (0.175 and 7.2° for Mercury²², 0.242 and 15.9° for Pluto²³), as well as the high Moon: Earth mass ratio, all indicate that the population of planetesimals included some sizable bodies, perhaps even larger than the Moon. Related to this is the apparent over-stability of the Solar System: the generous spacing between the planets which is the essence of Bode's law. From models which depend on relatively close interaction for dynamical evolution^{17,18}, it is difficult to avoid the conclusion that there should be several more planets than observed. The clearing out of the intermediate bodies must have depended on longer term perturbations associated with the perihelion and node revolutions. Bodies midway between successive terrestrial planet extreme aphelia and perihelia with eccentricity oscilla-

tions comparable to those in the present system²² would not survive. Bodies between major planets would need eccentricity oscillations three or four times those of major planets to collide with the planets.

The principal message is that collision with pre-existing satellite matter is the most effective means of capturing a moon, and that the "normal" class of models of evolution from dust through planetesimals to planets would have led to circumstances where matter was orbiting about protoplanets. There are limits, of course, to how much energy can plausibly be expended by collision. Furthermore, the farther away from the Earth a body originates, the narrower the range of conditions will bring it within a capturable energy relative to the Earth. Thus, for example, a near-minimum energy trajectory from Mercury to the Earth, required in Cameron's hypothesis⁴, will approach the Earth with a relative velocity of about 7.5 km s⁻¹ (2×10^{37} erg to dissipate); the distance of this approach will vary 10 Earth radii for a change of only 0.006 km s⁻¹ in initial velocity at Mercury.

Given that the Moon's matter arrived in orbit about the Earth in many chunks, the realistic alternatives are: (1) one chunk was much larger than the rest, perhaps considerably more than half the total mass; and (2) all the chunks were small compared with the total mass of the Moon. Perhaps the best evidences of the latter are that the outer half of the Moon is severely depleted in volatiles and enriched in refractory silicates, and that at least the outer fourth of the Moon was heated sufficiently to differentiate a crust very soon after its formation^{1,2}. Given that there were volatiles entrapped when the materials of certain bodies (such as the Earth and the parents of carbonaceous chondrites) were last put together into, say, 10 m or larger pieces, then the only way to remove it thoroughly from other matter in the same part of the Solar System is to break the latter down into small pieces, requiring many collisions. Furthermore, the only certain way to achieve the primordial heating evidence in the Moon's outer parts is by rapid infall in the terminal stages^{24,25}. Both these circumstances—frequent collisions and rapid terminal infall—could not be obtained on the relatively long time scale of formation in heliocentric orbit^{17,18}, but could be on the short time scale of formation in geocentric orbit. As discussed by Ruskol²¹, there were probably repeated cycles of coalescence and fragmentation of the circumterrestrial matter, because of repeated disruption by further matter falling in from outside the Earth-Moon system. In this manner, the formation of the Moon may have been delayed as much as 10^8 yr. But once the protolunar matter was left undisturbed, the formation would have proceeded rapidly. To give the initial inclination between the lunar orbit and the Earth equator, one or a few fair-sized planetesimals (less than 1/10 lunar mass) must have hit the Moon or the Earth from outside the Earth-Moon system after the Moon was formed²⁶.

A circumterrestrial regime of repeated collisions would certainly have been conducive to loss of volatiles and enrichment in refractory materials^{27,28}. The depletion of the protolunar matter in iron relative to the Earth may have been due, however, to an earlier differentiation in the nebula, favouring enrichment of iron in earlier accreting bodies for either thermochemical²⁹ or mechanical³⁰ reasons.

Dynamical plausibility suggests that satellites have different compositions than their associated planets both because of later formation and because of the frequent collisions and rapid coalescence in a circumplanetary swarm. It also suggests that collisions are the most effective means of energy dissipation for orbital changes; tidal friction is too slow, and gravitational effects are too temporary. The retrograde satellites of Jupiter are sometimes mentioned as examples of gravitational capture⁷; however, they are too far inside the marginal Jacobi constant³¹, and collisions seem the most reasonable explanation even for their capture³².

Our notion of plausibility entails minimal change from the present Solar System, of course. If the solar nebula had a mass

on the order of a solar mass, as hypothesized by Cameron³³, most of the dynamical considerations discussed here would not constrain events before the dispersal of the nebula by the T Tauri phase of the Sun. One difficult thing to understand about Cameron's model is that if the solid matter in the central plane of the nebula was dense enough to allow growth of major planets in a few thousand years, then in this same time most of the matter should have collected by gravitational instability into bodies of radius a few kilometres^{17,18}, too large to be carried by gas drag or to be swept away by any T Tauri wind. Furthermore, the number of these bodies and the high gas density would have led to many Moon sized bodies, and perhaps some terrestrial planet sized, too large to be broken up by collision. In other words, it is difficult to make the time scale of gravitational instability of solid matter longer than the time scale of gravitational capture of gas, as would be necessary if Cameron's massive nebula has a solar proportion of iron, silicates and so on.

A possible necessary condition strongly affecting the solar nebula and planet formation therefrom may be the short term existence of other stars only a few tens of astronomical units away. The cloud collapse calculations of Larson^{34,35} indicate that if angular momentum is retained, multiple star systems result. Such systems are often unstable, usually ejecting one member at a time until a binary, or, less often, a stable triplet is attained³⁶. If isolated stars such as the Sun are normally ejected from multiple systems, then no solution of planet formation, angular momentum distribution, and so on, would be complete without considering the influences of other stars in the system.

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Particles in the Polar Stratospheres

SINCE 1971, the University of Wyoming's atmospheric physics group has been monitoring the stratospheric concentration of submicron particles globally using the balloon sounding technique with a dustsonde¹. Included in this programme are annual soundings from the ice island T-3, at present at about 85° N, and from McMurdo Station (78° S) and South Pole Station (90° S) in Antarctica. For reasons of accessibility measurements are made in both polar regions at approximately the same time—northern hemisphere winter. Thus the measurements were carried out at T-3 during December 1971 and December 1972 and at the South Pole during January 1972 and January 1973. We describe the results here.

Figure 1 shows a comparison of the vertical distribution of particles with diameter greater than 0.3 μm from the ice island T-3 taken 1 yr apart. Above an altitude of about 15 km, little change is seen. Below this altitude the observed decrease is similar to that observed at northern mid-latitudes during this period². High concentrations of particles in the lower atmosphere (below 9 km) were again observed in 1972, although not as extensively as in 1971. The average particle size below 9 km was observed to be smaller than in the stratosphere, probably indicating a different particulate source for the two regions.

Measurements of particles of the same size made 1 yr apart at the South Pole are shown in Fig. 2. Except for the sharp layer at an altitude of about 3 km in the 1973 sounding, which was due to a low-lying cloud over the 2.8 km polar icecap, there is little change at any altitude. The same conclusion is reached if one compares soundings made from McMurdo and the South Pole at about the same time as shown in Fig. 3. Because no significant injections of particles into the stratosphere by volcanoes or other means have been reported during this period, profiles such as those in Figs 2 and 3 may be indicative of the "natural" (non-volcanic) stratospheric aerosol background in the polar regions.

The similarity of the vertical particle profiles in the two polar regions, as shown in Fig. 4, suggests a common production mechanism for the particle layer and thus indicates that the source is probably not due to local human activity as such

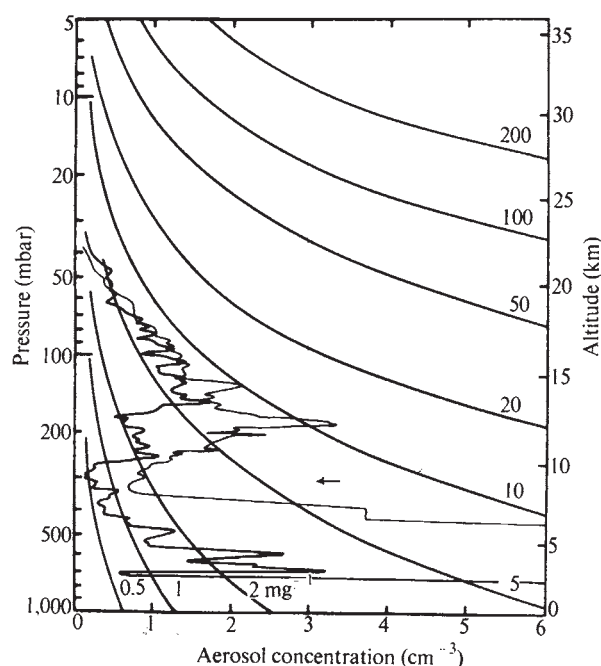


Fig. 1 The vertical distribution of aerosol particles having diameters greater than 0.3 μm over the ice island T-3 (85° N) on two occasions 1 yr apart. ←, Observed tropopause position in 1971. Smooth curves are lines of constant mixing ratio in units of particles per mg of air. Light trace, December 3, 1971; heavy trace, December 6, 1972.

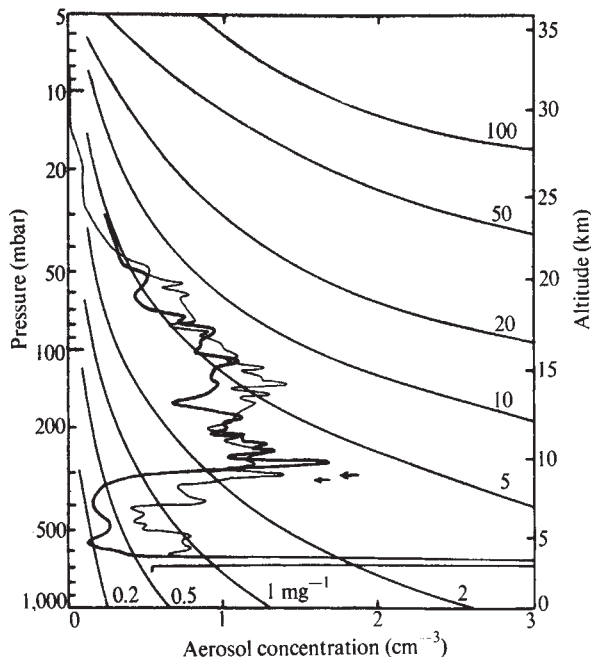


Fig. 2 The vertical distributions of aerosol particles having diameters greater than $0.3 \mu\text{m}$ over the South Pole on two occasions a year apart; light trace, January 24, 1972; heavy trace, January 16, 1973. $\leftarrow$, Observed tropopause positions. Smooth curves are as in Fig. 1.

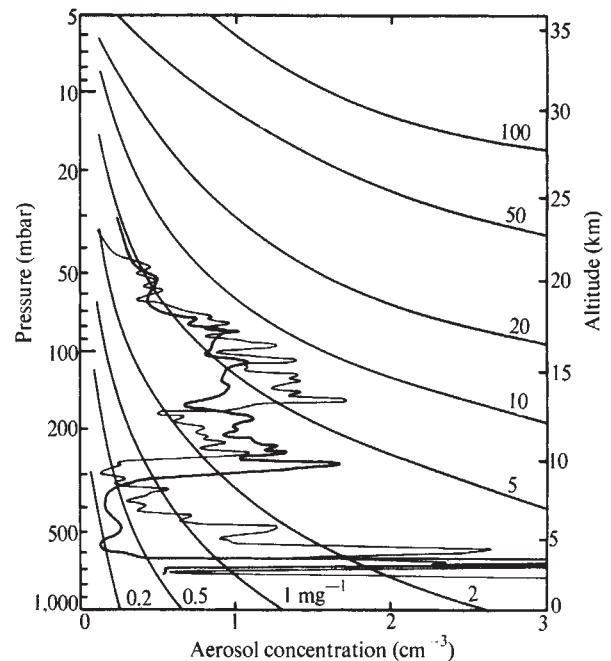


Fig. 4 The vertical distributions, 6 weeks apart, of aerosol particles having diameters greater than $0.3 \mu\text{m}$ over the ice island T-3 (light trace, December 6, 1972) and the South Pole (heavy trace, January 16, 1973). Smooth curves are as in Fig. 1.

activity is substantially more pronounced in the northern hemisphere (polar jet flights, for example) than in the southern hemisphere and as substantial exchange across the equator is thought not to occur. Figure 4 also suggests that there is little seasonal variation in stratospheric particle concentration at the poles, for one sounding was taken in local winter and one in local summer; this suggestion has not, however, been verified. This would be in contrast to the situation at lower latitudes, for the monthly measurements at northern mid-latitudes for the past 1.5 yr in this programme suggest an increase in the total stratospheric particulate loading in the lower stratosphere during the winter with a consequent removal in spring and a

minimum loading in summer. The source of such increases is unknown. The decreases seem, however, to be related to extensive particle layers in the vicinity of the tropopause and may result from loss through the tropopause gap at mid-latitudes. Thus unless the polar seasonal variation is out of phase with that at mid-latitude by 3 months, it probably does not exist.

The marked similarity of the stratospheric particle distribution in the two polar regions has other interesting implications. Particle composition analysis by several research groups has indicated that the principal elemental component of the aerosol in the lower stratosphere is sulphur (for a review see ref. 3 or 4), hence the commonly used term "sulphate layer". The sulphur can enter the stratosphere in several ways—by volcanic injection in the form of sulphur-bearing gases or sulphate compounds; by natural injection of tropospheric sulphur-bearing gases, for example by upward convection in tropical regions; or by the influx of tropospheric sulphur-bearing Aitken particles (diameters $<0.1 \mu\text{m}$).

The first two sources could involve gas phase reactions to form particles whereas the latter involves agglomeration. A higher northern hemisphere particle loading would perhaps be expected if one of the latter two sources was effective, because the northern hemisphere seems to be the dominant sulphur producer by at least a factor of two⁵. This is due to a combination of an excess of pollutants (93% of man-made SO_2 is produced in the northern hemisphere⁶) and a larger land mass in the northern hemisphere. Volcanic injection is not a likely source of these particles (at least of those above about 15 km) since the vertical distribution at the South Pole did not change substantially in 1 yr. Thus either the dynamics of the problem or the sulphur cycle itself is not well understood.

In regard to the dynamical problem, one might assume that the polar stratospheric aerosol is unaffected by aerosol transport from lower latitudes (as would be expected if the mid-latitude sink already mentioned is always operative). If this were the case, then the polar tropospheric sulphur source would have to be local, since Junge⁷ has observed that the sulphate content of ice samples from Greenland remained constant from 1915 to 1957, which is interpreted to mean that the increased industrial SO_2 output during this period could not penetrate into the Arctic. Fenn *et al.*⁸ and Cadle *et al.*⁹

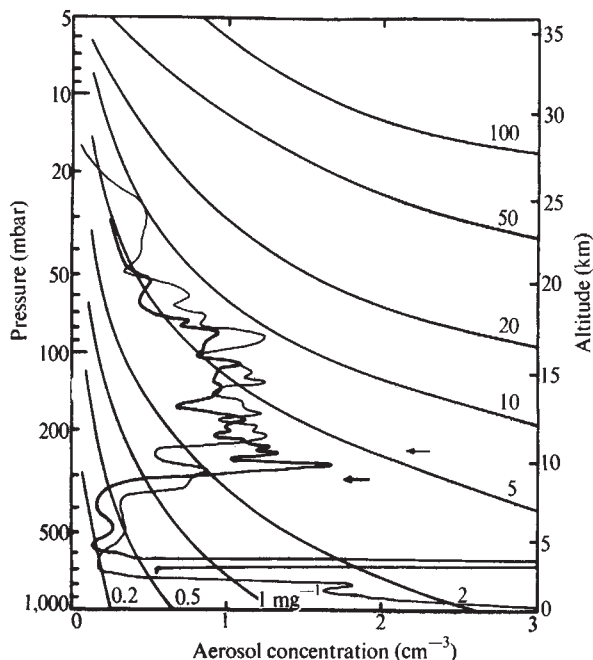


Fig. 3 The vertical distributions, 4 d apart, of aerosol particles having diameters greater than $0.3 \mu\text{m}$ over McMurdo, Antarctica (78°S) (light trace, January 12, 1973) and the South Pole (heavy trace, January 16, 1973). $\leftarrow$, Observed tropopause positions. Smooth curves are as in Fig. 1.

have shown that in the Arctic and Antarctic respectively the aerosol content of surface air has a much larger relative sulphate concentration.

One possible source of the sulphate⁹ is downward mixing from the stratospheric sulphate layer. If such a simple model is correct, then a somewhat clearer picture emerges, with the observed polar particle profiles being an equilibrium distribution due to the natural local source (gas phase reactions, for example) and diffusion processes and sedimentation. With such a model, the degree of sulphur compound input to the stratosphere would depend only on the local surface characteristics and sources in the polar regions, which are not too different in the two hemispheres, accounting for the similarity in the stratospheric observations. The stratospheric concentrations observed in polar regions are, however, at least as great as those observed in mid-latitudes and somewhat higher than those in low latitudes, which would not be expected with such a model.

Regarding the sulphur cycle, the oceans may play a larger role in the global sulphur budget than previously assumed. For example, the dimethyl sulphide $[(CH_3)_2S]$ discovered in sea water by Lovelock *et al.*¹⁰ may not only be the missing ocean sulphur source (previously thought to be H_2S), but could prove to be an important source for stratospheric sulphate aerosol. Although the end product of SO_2 or H_2S gas phase reactions in the stratosphere, important for aerosol formation, is sulphuric acid, the equivalent product for $(CH_3)_2S$ is methane sulphonic acid, which has physical properties similar to sulphuric acid. Thus detection of the organosulphur acid in the stratospheric aerosol would be strong evidence for the importance of the maritime methyl sulphide in the global sulphur budget.

To resolve the problem of the natural stratospheric particle source, further stratospheric particle measurements during all seasons in both hemispheres and additional global SO_2 , H_2S and $(CH_3)_2S$ measurements, both on the surface and as a function of altitude, are required.

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depression near the foot of the eastern wall at latitude $36^{\circ} 49.3' N$ and longitude $33^{\circ} 15' W$. Two types of rocks were recognized from their structure and their degree of weathering. One type is "slab-like" rock, 2 to 13 cm in diameter and 1 to 2 cm thick (Fig. 1a and b). The other type of rock consists of oval glassy pebbles (1 to 5 cm in diameter, Fig. 1c) which were observed to jump and explode, one or two at a time, for 3 d after the dredge haul was emptied on to the deck of the ship.

Here we report two volcanic phenomena which have been only rarely described in the literature from first hand experience, namely, the occurrence of lava tubes in the ocean and the observance of "popping rocks". To our knowledge, popping rocks have only been reported twice before; once from the southern part of the continental slope west of Baja, California, by Krause¹ and once from the Confederation Peak area at $45^{\circ} N$ on the Mid-Atlantic Ridge by Aumento².

The lava slabs are slightly concave on one side, with a thin film of manganese and iron oxide coating, whereas the convex side is rugose and sometimes shows wrinkles indicating the direction of lava flow (Fig. 1b). The convex surface consists of a palagonite crust with small amounts of fresh glass (Fig. 1a). Both surfaces of the slabs are weathered but the inner portion is fresh (Fig. 1b). The slabs are primarily made up of basaltic glass which becomes more crystalline towards the concave side. The most crystalline portion of the slabs has tiny, poorly crystallized laths of what is probably plagioclase and traces of small euhedral olivine. Centres of crystallization with polygonal outlines rimmed with glassy veins have also developed in the less glassy portion of the rock. We think that these slabs are fragments of lava tubes which have collapsed after drainage. The side of the arched surface shows the occasional occurrence of small ridges which are 3 mm high and 3 mm thick. A similar structural feature has been found on the internal walls of lava tunnels from Mount St Helens in Washington and was attri-

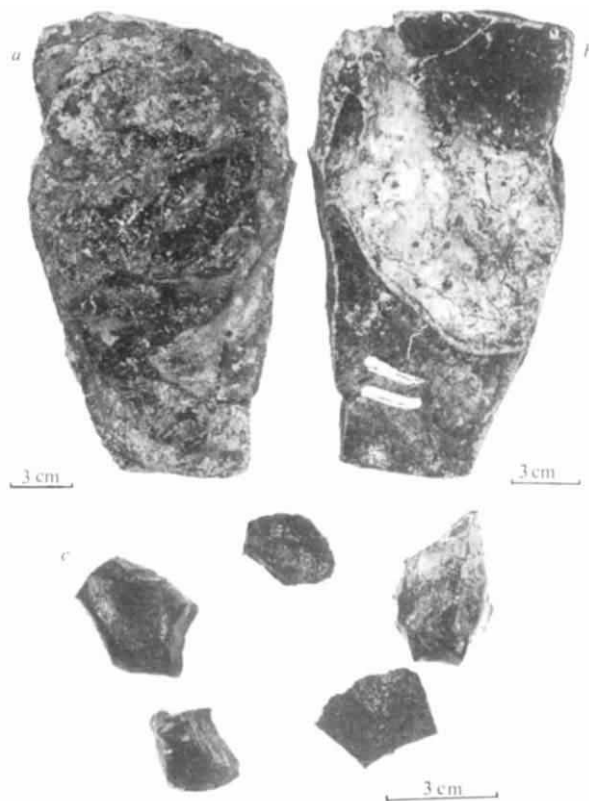


Fig. 1 a, Convex surface of a slab fragment showing glassy zones (dark), palagonite, and a thin film of manganese; b, concave side of a slab fragment showing thin ridges, manganese coating (dark area), sediment (light area), and traces of organisms (dark streaks); c, angular fragments of "popping rocks"; some show a concave breakage surface.

Popping Rocks and Lava Tubes from the Mid-Atlantic Rift Valley at $36^{\circ} N$

DURING the 1972 Midlante cruise of the RV Jean Charcot to survey a limited area of the rift valley south of the Azores, a dredge (CH-DR11) was taken at a depth of 1,360 fathoms in a

Table 1 Chemical Analyses and Norms of a Popping Rock (315X) and of a Slab (315Y) from the Mid-Atlantic Ridge Rift Valley at 36° N

Dredge No. Sample No.	CH31-DR11 315X	CH31-DR1 315Y
SiO ₂	49.68	48.74
Al ₂ O ₃	14.70	14.57
Fe ₂ O ₃	1.52	1.86
FeO	10.30	10.55
MgO	8.15	8.16
CaO	11.77	11.70
Na ₂ O	2.29	2.15
K ₂ O	0.22	0.23
TiO ₂	1.44	1.43
P ₂ O ₅	0.18	0.18
PF 110° C	0.07	0.05
PF 1,050° C	0.58	0.47
Total	100.89	100.08
Norms		
Q	1.92	2.19
or	1.30	1.35
ab	12.10	11.76
an	33.04	32.83
di	19.81	19.70
hy	25.85	26.12
mt	2.20	2.69
ilm	2.73	2.71
ap	0.42	0.42

PF ignition.

Analyst: P. Cambon.

buted to the remains of lava which solidified along the walls during drainage⁸.

The exploding rocks found in the same dredge as the lava tubes jumped approximately 50 to 100 cm while making a loud noise similar to that of popping corn. Each pebble shattered into at least two pieces having a conchoidal fracture and concave breakage planes (Fig. 1c). The rock fragments are black and glassy and contain occasional skeletal olivine (<0.07 mm in length). A grill-like network of what seems to be incipient plagioclase needles abounds throughout. Vesicles less than 1 mm in diameter comprise about 3% of the bulk rock.

The chemistry of a popping rock (sample 315X) and of a lava tube (sample 315Y) is shown in Table 1. The analysis of the lava tube was made on the fresh interior of the slab. Both types of rocks are low alumina tholeiites which are over-saturated with respect to silica. These rocks have a similar composition to other Mid-Atlantic ridge basalts^{2,4-6}. Ignition (H₂O + CO₂) gave values less than 0.7% for both types of rocks (Table 1). After heating the samples to 110° C only minor

Table 2 Volume Percentage of Gases of the Popping Rock (DR11-315X) and of a Glassy Margin of a Pillow-lava Fragment from the Rift Valley at 36° N

Volume %	DR11-315X	DR1-112
HCl	6.6	6.5
CO ₂	38.8	44.0
CO	16.8	12.6
SO ₂	10.3	1.7
H ₂ S	0.2	None
CS ₂	None	Trace
H ₂	26.7	34.7
Methane	0.2	0.2
Ethane	Trace	Trace
Propane	0.2	Trace
Butane	Trace	Trace
Ethylene	Trace	Trace
Propylene	Trace	Trace
Butene	Trace	Trace
C ₆ H ₆	Trace	Trace
Helium	None	None
N ₂ + Ar	0.1	0.2

Sample DR1-112, located at 36° 45.65' N and 33° 16.55' W (1,337–1,408 fathoms) is a tholeiite with a few olivine crystals and with less than 10% vesicles (<1 mm in diameter). The HCl in the released gases was determined separately, using silver phosphate as a reagent and measuring the amount of silver chloride formed.

amounts of volatiles (0.06%) escaped. Most of the volatiles escaped when heating the sample to 1,050° C. This suggests that most of the volatile content was concentrated within the glass and represents the original amount of volatiles present in the lava during extrusion.

Unfortunately we did not have to hand an appropriate container in which to bring back some of the popping rocks before they exploded; the amount and types of gases released during the explosions are thus not precisely known. A laboratory study of an exploded popping rock reveals the presence of various gases and the results of the mass spectrometry study are shown in Table 2. In order to compare the gas content of the popping rock with that of another oceanic basalt which did not explode on the ship's deck, analyses of gases were also done on some fresh glass representing the chilled margin of a tholeiitic pillow-lava fragment (DR1-112) dredged from the same general area of the Mid-Atlantic Rift Valley (Table 2).

To extract gases from the two samples, 10 g of fragments varying in weight from 0.3 to 0.6 g were first gradually heated in a vacuum to a temperature of 1,000° C and then ground into a fine powder (<0.125 mm in diameter); 10 g of the powder was again gradually heated in a vacuum to 1,000° C to release still more gases from the glassy network. Observations made during the discharge of the gases are shown in Table 3. Small explosions were noted in both samples although the popping rock had a larger temperature range (180 to 400° C) than did the glassy margin of the pillow lava (400 to 450° C) (Table 3). The total volume of gases evolved from the popping rock (DR11-315X) is higher (0.881 ml g⁻¹) than the volume released from the glassy margin of the pillow lava (0.734 ml g⁻¹) (Table 3). The volume of gas evolved from both tholeiitic glasses is four times greater than the gas content of a continental obsidian from Lipari which released only 0.281 ml g⁻¹ (ref. 7).

The volume percentage of gases released from the two samples is shown in Table 2. Traces of hydrocarbons were observed in both samples. The chief differences noted between the two samples are in their SO₂, CO, CO₂ and, to a lesser extent, their H₂ contents. The popping rock has a considerably higher SO₂ content (10.3%), a higher CO content (16.8%), and a lower H₂ (26.7%) and CO₂ (38.8%) content than does the glassy margin of the pillow lava fragment. Interestingly, there was no smell of sulphur as the popping rocks exploded during 3 d on the ship's deck. The lower H₂ and CO₂ contents observed in the laboratory comparison of the popping rock and the glassy margin of the pillow lava may indicate, however, that the CO₂ and H₂ were responsible for the explosion of the popping rocks on the ship.

This latter consideration is valid if we assume that the original gas contents of both the popping rocks and the glassy margin of the pillow lava, on solidification, were initially the same. The popping rocks exploded when gases were released along lines of weakness near the surfaces of the fragments. This release of gas was probably a result of the sudden change in pressure and temperature when the rocks were brought to sea level. We believe, however, that the change of ambient pressure

Table 3 Laboratory Observations During Degassing Procedure on Two Rift Valley Samples from 36° N in the Atlantic Ocean

	DR11-315X		DR1-112	
	Coarse fraction	Powder	Coarse fraction	Powder
Temperature at the beginning of gas release (° C)	217 to 220	255 ± 5	260 ± 5	265 ± 5
Volume of gas released at 0° C and 760 mm Hg	0.672 ml g ⁻¹	0.209 ml g ⁻¹	0.587 ml g ⁻¹	0.14 ml g ⁻¹
Weight loss (%)	0.56	0.09	0.45	0.08
Approximate temperature range of explosion (° C)	180 to 400	None	400 to 450	None

during the emplacement of the lava flow on the floor of the Rift (250 to 300 bar) to that of sea level (1 bar) is the most important factor governing the explosions observed on deck. The explosive nature of these glassy basalts suggests an extremely recent extrusion of lava and it seems, in fact, that the lava flow had not yet had time to come to equilibrium with the pressure and temperature conditions of the rift floor. The presence of only a few skeletal olivine crystals indicates that the quenching temperature of the lava was near to that of olivine crystallization which is greater than 1,100° C at 250 to 300 bar.

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Magnetic Telechemistry of Oceanic Crust?

It is now established that basalts from Iceland¹⁻³, the Galapagos Islands⁴, and other volcanic island and seamount^{5,6} chains in general, are richer in iron and titanium than normal abyssal tholeiites produced by seafloor spreading on the Mid-Oceanic Ridge. According to present thought, these chains are the volcanic derivatives of mantle plumes over which the lithospheric plates make their way⁷. Schilling² recently showed progressive decreases in minor and trace element concentrations along the Reykjanes Ridge south-west away from Iceland; he interprets this as the result of mixing of a plume and a normal, depleted asthenosphere below the spreading axis, as the Icelandic asthenosphere travels southwards in a pipe or conduit below the spreading axis⁸. The model has been challenged⁹, but the geochemical variations seem to be characteristic of plumes the world over. From an economic point of view, mid-oceanic hydrothermal muds and brines formed near plume centres would seem best suited as mineral reserves because of the inherently higher concentration of various metals in the upper crust. Is there any way to make a rapid assay of the ocean crust to identify such regions without tedious dredging and chemical analyses?

The answer is yes, in view of the higher Ti and Fe in the plume-derived ocean crust. Because oceanic magnetic anomalies are essentially the result of thermoremanent magnetisation carried by finely disseminated titanomagnetite¹⁰, a basalt crust with higher Fe and Ti should have higher amplitude magnetic anomalies, other factors being equal. This assumes a relatively fixed distribution ratio of Fe and Ti contained in titanomagnetite to that contained in other minerals in which these elements are found.

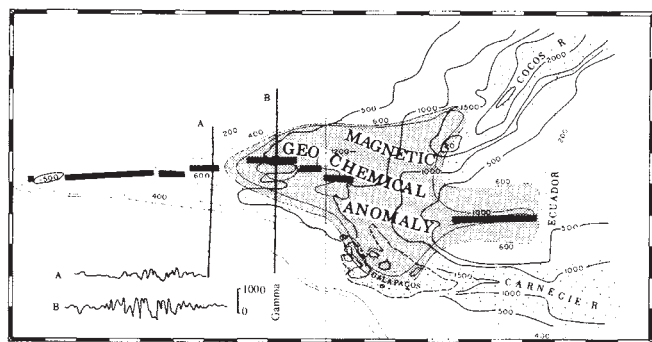


Fig. 1 Maximum peak-to-trough magnetic amplitudes (light lines) and residual basement elevation (heavy lines, in m above North Pacific reference of Sclater *et al.*¹⁸). Magnetic amplitude contours (gamma) based on maximum magnetic anomaly relief within each 30 mile square, as constructed from available profiles (P. R. V. and G. L. J., to be published). Residual basement elevation was obtained by averaging basement depth in each 30 nautical mile square, correcting for sediment loading with the help of published data¹⁹, and comparing with North Pacific crust of comparable age¹⁸, using R. Hey's magnetic isochrons for the Galapagos crust (thesis in preparation). Dotted Cocos and Carnegie ridges may be geochemically anomalous despite lack of large amplitudes. Regions below 200 gamma were formed by roughly east-west spreading from East Pacific Rise. Transition between plume and normal crust occurs between magnetic residual profiles A and B; the difference between the amplitudes of these two profiles cannot be explained by the relatively small (200 m) source depth difference. South is on left for profiles.

At least three plume areas support the feasibility of "magnetic telechemistry": The Juan de Fuca Ridge contains one or more plumes, whereas the nearby Gorda Ridge does not⁷. Dredge samples from the Juan de Fuca Ridge contain an average of 12.5% total Fe as FeO; the nearby Gorda Ridge contains only 9.2% (ref. 11) (Fig. 2). On Iceland FeO reaches 15% and TiO₂ 4% (ref. 1), the latter roughly four times the typical oceanic value reached 500 km south-west of Iceland on the Reykjanes Ridge^{2,3}. Some of the youthful main volcanoes of the Galapagos contain 2 to 4% TiO₂ and total Fe as FeO is 11 to 15% (ref. 4). JOIDES drilling on the Carnegie and Cocos aseismic ridges (Fig. 1) showed 12.43% Fe₂O₃ and 2.15% TiO₂, and 12.35% Fe₂O₃ and 1.50 TiO₂, respectively¹². These ridges represent volcanic trails left as the Cocos and Nazca plates moved over the Galapagos plume¹³. Thus, "pure" plume-derived ocean crust may be expected to contain about one-third more iron and several times more titanium than normal ocean crust.

Do magnetic anomaly amplitudes on ocean crust near plumes reflect a difference of this magnitude? We have determined the maximum measured magnetic anomaly relief within each 30 nautical mile square and contoured this quantity, which will be a rough measure of magnetisation intensity, relatively independent of spreading rate, after differences in water depth and geomagnetic orientation are allowed for and if the magnetised layer does not vary in thickness. In Fig. 1 we show this together with the basement elevation anomaly for the Galapagos plume and nearby spreading axis; detailed analyses are in preparation, but it is clear already that a section of the Galapagos spreading centre within 500 km of the plume centre has peak-to-trough magnetic anomalies of 1,000 to 1,200 gamma in the axial region and 800 to 1,000 gamma on the flanks; outside this region (shaded in Fig. 1), the amplitudes drop to 400 to 600 gamma. Published magnetic data¹⁴ show that the Juan de Fuca Ridge has seafloor spreading anomalies of 800 to 1,400 gamma, which drop to 500 to 900 gamma on the Gorda Ridge south of the Blanco fracture zone¹⁴ (Fig. 2). Within 1,000 km west of a proposed plume or group of plumes on the Pacific-Antarctic Ridge south of Tasmania¹⁵ average peak-to-trough magnetic anomaly amplitudes¹⁶ range from

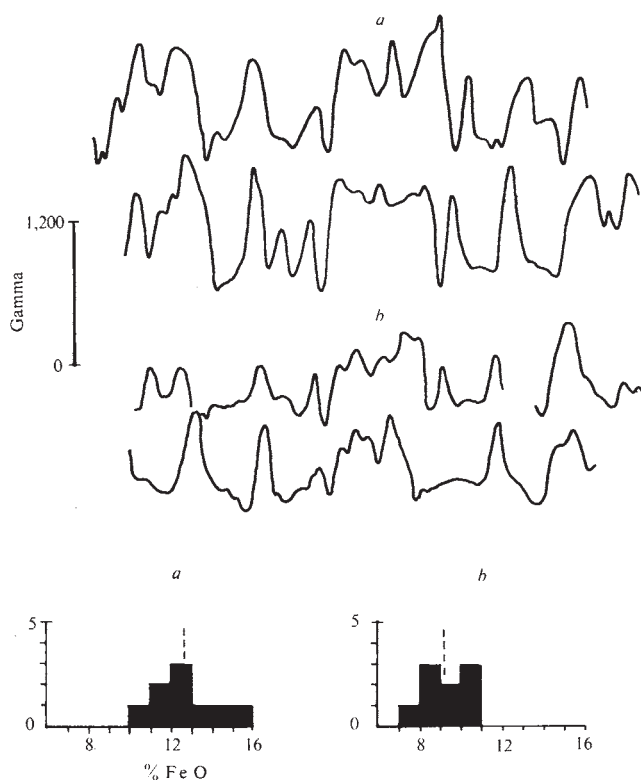


Fig. 2 Residual magnetic anomaly profiles across Juan de Fuca (a) and Gorda (b) Ridges (lines 8, 18, 34 and 36 of ref. 14, from top to bottom) and total iron as FeO from dredge samples of the two ridges¹¹. Dashed vertical lines show mean FeO of 12.6% and 9.2%. Basement depths, 2.2 to 2.9 km, 2.2 to 3.3 km, 2.5 to 3.5 km, and 2.5 to 3 km along the four profiles¹⁴ cannot account for the differences in magnetic anomaly amplitude between the Juan de Fuca and the Gorda Ridge.

1,500 gamma at the ridge axis to 600 gamma at anomaly 5 (Fig. 3). Further west, south of western Australia, with no plume nearby, the corresponding values are only 800 and 300 gamma. Because the anomalous amplitudes date from 10 m.y. BP only, either the plume was less active before

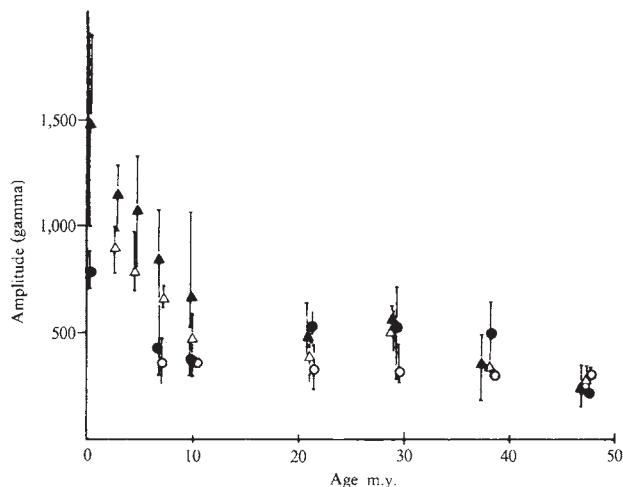


Fig. 3 Average peak-to-trough amplitudes of key magnetic anomalies south of Australia¹⁶. Triangles show zone A (ref. 16) on Mid-Oceanic Ridge close to proposed Balleny-Tasman plume or plume complex¹⁷. Zone C anomalies (circles) are distant from nearest plume. Bars show range of observations. Open symbols are anomalies south of spreading axis, closed ones north of axis. Note higher amplitudes on north side of ridge. Does this imply that two parent asthenospheres² are unevenly mixed, or some other asymmetry in the magnetisation process?

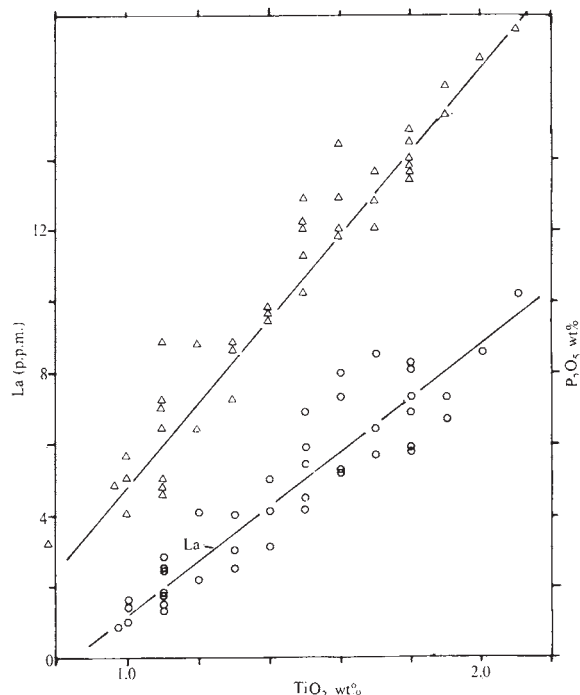


Fig. 4 La and P₂O₅ concentrations for Reykjanes Ridge and Iceland are plotted against TiO₂ from data of Schilling² to illustrate how, if Ti or Fe content of plume-type ocean crust could be estimated from magnetic anomalies, various other elements can be estimated also. A more complete list of anomalous elements and isotopes is given in ref. 2.

or the "plume crust" loses its magnetisation more readily. Near Iceland the anomaly amplitudes are reduced (in spite of higher Fe and Ti¹⁻³), probably the result of increasing ash content and fracturing of the lavas emitted at low hydrostatic pressures in shallow water (P. R. V. and G. L. J., to be published). The Iceland plume is, however, probably unique in this respect.

When attenuation and other known effects are considered, the magnetisation of plume-derived ocean crust must be 50 to 100% higher than that of normal ocean crust nearby. This difference is comfortably intermediate between the roughly 33% FeO increase and the 100 to 300% TiO₂ increase of plume over normal basalts deduced from geochemical measurements.

Preliminary analysis thus suggests a new application for magnetic anomalies, namely as an indicator of crustal geochemistry. Linear regression of Schilling's² published Sm, La, P₂O₅ and K₂O against TiO₂ shows that the concentration of these other elements can be nicely predicted once Ti (or Fe) is known, and additional anomalous elements (Rb, Ba, Cs, U, Th)² and Pb and Sr radiogenic isotopes will no doubt follow suit. Figure 4 shows La and P₂O₅ as examples.

One can make an optimistic analogy with assays of ocean water chemistry. Electrical conductivity is measured and correlated with chlorinity, which then specifies many common constituents of seawater because they maintain constant ratios. By the proposed method of "magnetic telechemistry" one measures magnetic anomalies, which can be done by air, and then correlates amplitudes with Fe and Ti content of the underlying crust. One could then use magnetic anomalies to specify a whole suite of elements and isotopes that vary as Fe and Ti (Fig. 4). Of course, this is only an idea; much more work is required to establish the exact relationships. Fortunately the method is relatively independent of the mechanism of Fe-Ti enrichment, whether it be Schilling's² or some other⁹.

Clearly the anomaly amplitudes could also reveal the extent of past plume influence on the ocean crust. Such data could furthermore test the proposed subaxial conduit

convection¹⁷, for the anomalous crust would then exist on only the plume side of a major transform. Convection at shallow depths below the Mid-Oceanic Ridge would be seriously impeded or arrested by the thick plate on the opposite side of the transform fault¹⁷. Possible effects of this kind are the termination of the Juan de Fuca region of high amplitudes against the Blanco fracture zone¹⁴, and the termination of the Galapagos high amplitude region against the Equador fracture zone. Finally, the observation that two nearby spreading centres can generate magnetic amplitudes different by a factor of two or three suggests that some "magnetic smooth zones" may simply reflect low Ti and Fe concentrations.

Since the Mid-Oceanic Ridge is elevated near plumes, one might try to correlate the topographic anomaly with the geochemical and magnetic amplitude anomalies (P. R. V. and G. L. J., to be published). In the Galapagos and other plume areas, any such relationship will be complicated by the presence of aseismic, volcanic ridges which represent large topographic residuals (+1,000 to 2,000 m for the Carnegie and Cocos Ridges in Fig. 1) but tend to be magnetically quiet, possibly because magnetised layers of opposite polarity are superimposed, and because of non-magnetic tephra layers. In spite of these complications, it is clear that the Galapagos spreading axis itself rises to more than 1,000 m above the North Pacific standard¹⁸, and that the region of anomalous magnetic amplitudes is centred on the topographic anomaly of the spreading axis (near 90° W in Fig. 1).

Richard Hey's (Princeton University) contributions toward unravelling the spreading history of the Galapagos Ridge were valuable; thanks also to N. Hunt (Naval Oceanographic Office). The research was partially supported by the Office of Naval Research.

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Burning Waste Gas in Oil Fields

I WAS recently amazed by some night-time spacecraft photographs, exemplified by Fig. 1, that present graphic evidence of waste and pollution. These were obtained by the United States Air Force DAPP system which has sensors in the visible 0.4 to 1.1 μm band and an infrared imaging system in the 8 to 13 μm band (ref. 1 and J. L. McLucas, personal communication). The visible band sensor is capable of responding to very dim light with a controllable threshold (T. O. Haig, personal communication) and it provided these pictures. The lights of cities are clearly visible, as are the aurora, surface features illuminated by moonlight, and fires such as those caused by burning gas from oil fields and refineries. Much power is evidently being generated to light the cities of the world since the inhabited areas are clearly outlined. It is also apparent that, in the process of extracting liquid petroleum from beneath the surface of the Earth, an abundant gas supply has been discovered but is not used. Being unable to contain the gas or to transport it to a user, it is simply burnt.

Apparently, the burning of gases has been a standard practice since the earliest days of the oil industry. I witnessed it when I was employed in the oil fields of Montana in 1953. Earlier, in April 1933, aviators travelling by way of North Africa on their way to fly over Mount Everest, described "Abadan with its dozens of oil tanks, refining plant and the huge flame which marks the burning of waste gases"². A spectacular photograph of one such flame can be found in a recent issue of the *National Geographic Magazine*³.

I presume that individual oil companies have weighed up the cost of research into ways of using this gas and the saving that would be likely if an economical answer were found. Perhaps the problem is of such magnitude that it should be financed by government agencies in the interest of avoiding pollution and conserving resources.

The solution may lie in new technology such as that based on hydrogen as an intermediate energy storage agent⁴⁻⁶. Perhaps the answer is a transport method or even a relocation of large energy users to the vicinity of waste gas sources. If relocation is to be considered, Fig. 1 (and other DAPP pictures) illustrate the grouping of gas sources that would provide the economic motivation. I am told that the most visible concentration of gas fires is near the Persian Gulf, off the right edge of Fig. 1 (T. O. Haig, personal communication); the largest groups shown in the figure are, in order of size, in Algeria, Libya, Nigeria and at the Gulf of Suez. Numerous gas fires appear to be located east of Kazan, USSR, and others may lie between Kiyev and Kharkov. There are many bright spots accompanied by streaks indicating an overloaded DAPP sensor; each is located in a region known to yield petroleum and from this I infer that gas flames are the most powerful light sources in the 0.4 to 1.1 μm band.

Aside from the cities and fires, an irregular array of lights extends across Africa from about 4° to 12° N. This pattern is not similar to the patterns of population density or political units; it follows the belt of savannah (grass and scrub) bounded on the north by steppe and on the south by tropical rain forest. I surmise that these lights are caused by agricultural burning to clear land for cultivation. In a similar climate near Darjeeling at the boundary of jungle, Hooker⁷ observed (in 1846) that such "fires, invisible by day, are seen raging all around . . . at night (when) we were literally surrounded by them; some smouldering, . . . others fitfully bursting forth, whilst others again stalked along with a steadily increasing and enlarging flame . . ." This observation, although remote from the African savannah, lends some support to the hypothesis that the belt of lights results from traditional fire clearance operations by man. The vast pollution caused by this practice is well known to Himalayan travellers who must hope for a rainstorm so that they can see the mountains.

Some African cities can be located among the belt of lights in Fig. 1; these appear as clusters of small lights without the

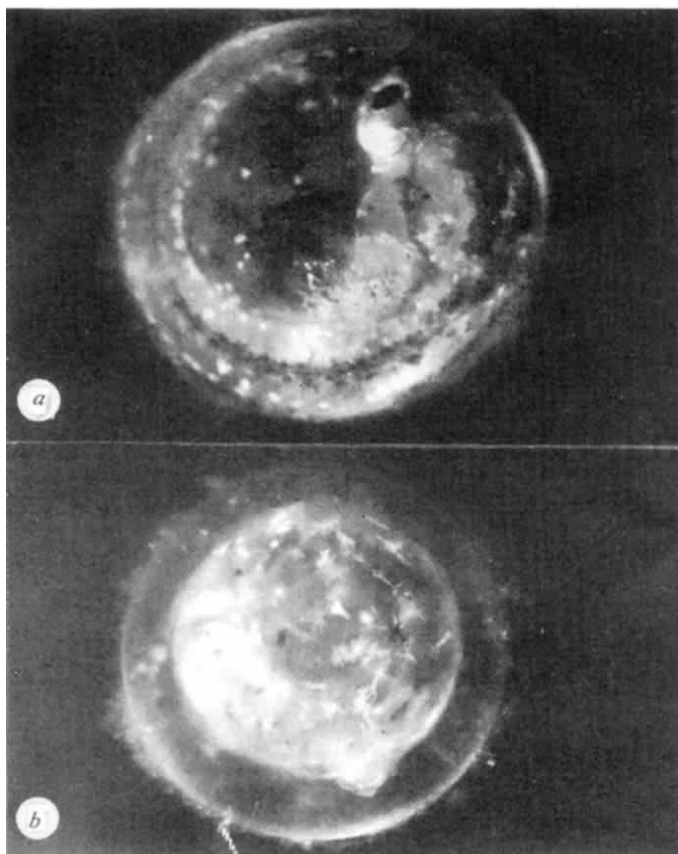


Fig. 1 Non-axiate embryo from treatment with actinomycin D and its control *Fundulus heteroclitus* (Linn.) was obtained in large numbers during the breeding season, June and July, in estuarine waters (Frenchman Bay, Maine). Eggs were stripped from ripe females into 50% sterile 'Millipore'-filtered (pore size 0.45 μ m) seawater containing 100 U penicillin and 50 μ g streptomycin ml^{-1} . Adequate sperm from sexually mature males was added for 1 min, when aliquots of zygotes were washed in and then placed in 50% seawater containing actinomycin D (20 μ g ml^{-1} , a gift of Merck and Co.). This concentration of actinomycin has been established by previous dose response curves and blocks approximately 50% of the total RNA synthesized^{6,7}. At the end of 1 h the zygotes were thoroughly washed and actinomycin D was completely removed from zygotes as established previously by stoichiometric measurements⁶. Embryos continued to be incubated in sterile 50% seawater at 16° C. This medium was changed daily. Development of both control (a) and actinomycin D-treated (b) zygotes was observed until late stages were reached. a, Normal control of the same stage of development. b, "Spherical tissue culture" or non-axiate embryo from experimental series.

relationships and migrate in cords and streams over the yolk. The periblast is equally disturbed. This results in a rough irregular blanket of cells covering the yolk as a "spherical tissue culture" (Fig. 1b) within which at a much later time there is some cellular differentiation (but not organ or organismic differentiation)⁶. Of first importance for our studies, only 5% of the treated embryos developed crude approximations of an axis after the short pulse with actinomycin D (Figs 1 and 2). Although there is an increased mortality with time, some of the non-axiate embryos survive until hatching of the fry, a period of 40 ± 2 d at 18° C.

To study changes in protein synthesis patterns in morphogenesis, radioactively labelled proteins were isolated from control and actinomycin D-treated embryos for electrophoresis on sodium dodecyl sulphate, 10% polyacrylamide gels. Since the morphogenetic defects in the experimental animals were known to be failures in normal gastrulation and in the formation of the antero-posterior body axis, it was anticipated that the banding patterns of the bulk proteins of actinomycin

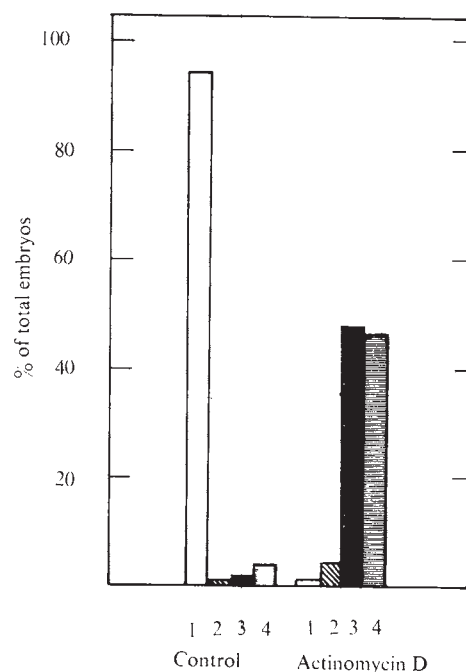


Fig. 2 Bar graph of results in morphogenesis of *Fundulus heteroclitus* zygotes treated with actinomycin D as described in Fig. 1. Development of the embryos (2,000) was characterized as (1) normal development, (2) minor anomalies (severe microcephalics), (3) major anomalies affecting axiation (no detectable axis) and (4) mortality (accumulated deaths during 20 d).

D-treated embryos collected at intervals after stage 10 (high blastula) would be similar to those of the normal (stage 10) controls. All bulk protein fractions from both experimental and control zygotes at stages 10, 15 (very early axis) and 19 (well established body axis) were, however, essentially identical. A representative gel is shown in Fig. 3. The acrylamide gel profiles of proteins in sea urchin embryos have also shown that bulk protein patterns are remarkably identical^{14,15} even though there is also demonstrable, new, specific protein synthesis¹⁶. While the incorporation of exogenous labelled amino acid into protein is very low during cleavage stages in *Fundulus* embryos, increases in incorporation following the blastula stage permit fractionation of synthesized proteins. Figures 3 and 4 show profiles of radioactive supernatant proteins. The sharp peak in all the profiles between fractions 0–10 has been identified, in part, as ¹⁴C-valine charged tRNA which runs with supernatant proteins and disappears after either hot trichloroacetic acid extraction or treatment with alkali. This treatment does not destroy any other radioactive peak. Experiments were compared by using this peak as a match-point and by slicing gels according to their stained band pattern.

Comparison of synthesized proteins from the normal developmental series, for example, high blastula (stage 10, Fig. 3a), gastrulation (stages 12–15, Fig. 3b) and early axiation (stages 16–19, Fig. 3c), showed a progressive increase in heavier single polypeptide chain synthesis. Principally in normal embryos of stages 15 and 19, newly synthesized polypeptide peaks (shaded regions) were detected between fractions 35–40 and 50–55, having an apparent molecular weight of 75,000 and 110,000 respectively. Proteins of lower molecular weight (10,000 to 50,000) such as are found in the high blastula (stage 10) apparently continue to be synthesized in later embryonic stages. The increased synthesis and diversity of heavier proteins with time in embryonic development are in agreement with studies using other embryos^{14,17,18}. When protein synthesized from either stage 15 or 19 control embryos was compared with actinomycin D-treated embryos of identical age, a shift in synthesis of higher molecular weight poly-

peptides was observed in the control embryos (Fig. 4a, b). Moreover, the older actinomycin D-treated embryonic protein profile (Fig. 4a, b) resembles very closely those of the younger but normal high blastula (Fig. 3a). At these stages, control embryos are in the process of completing the movements of gastrulation and entering the morphogenetic steps in the development of the body axis, while the actinomycin D-treated series are undergoing a form of cell movement at random on the surface of the yolk, with no normal morphogenetic potential. It seems clear that the morphogenetic processes of gastrulation and early axiation are associated with a broad area of new protein syntheses in the higher molecular weight range.

Antero-posterior axis formation is essential in normal morphogenesis and requires the coordination of manifold movements, positional interrelations and behavioural changes in the participating cells. Under appropriate conditions of inhibition of macromolecular syntheses, a serial order of defects of morphogenesis can be predicted which will reflect failure or partial failure of chemical processes essential to a

particular step in morphogenesis^{6,7}. Such a series of steps and defects can be used to trace the processes involved in normal morphogenesis, since as the time between fertilization and the onset of the inhibitor pulse (actinomycin D) is increased, gastrulation and axiation occur and the degree of anomalous

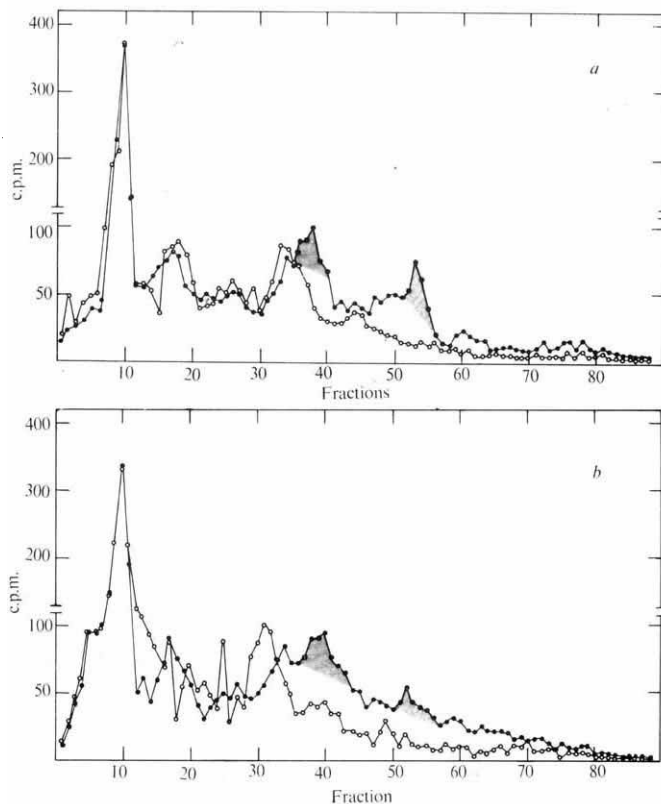
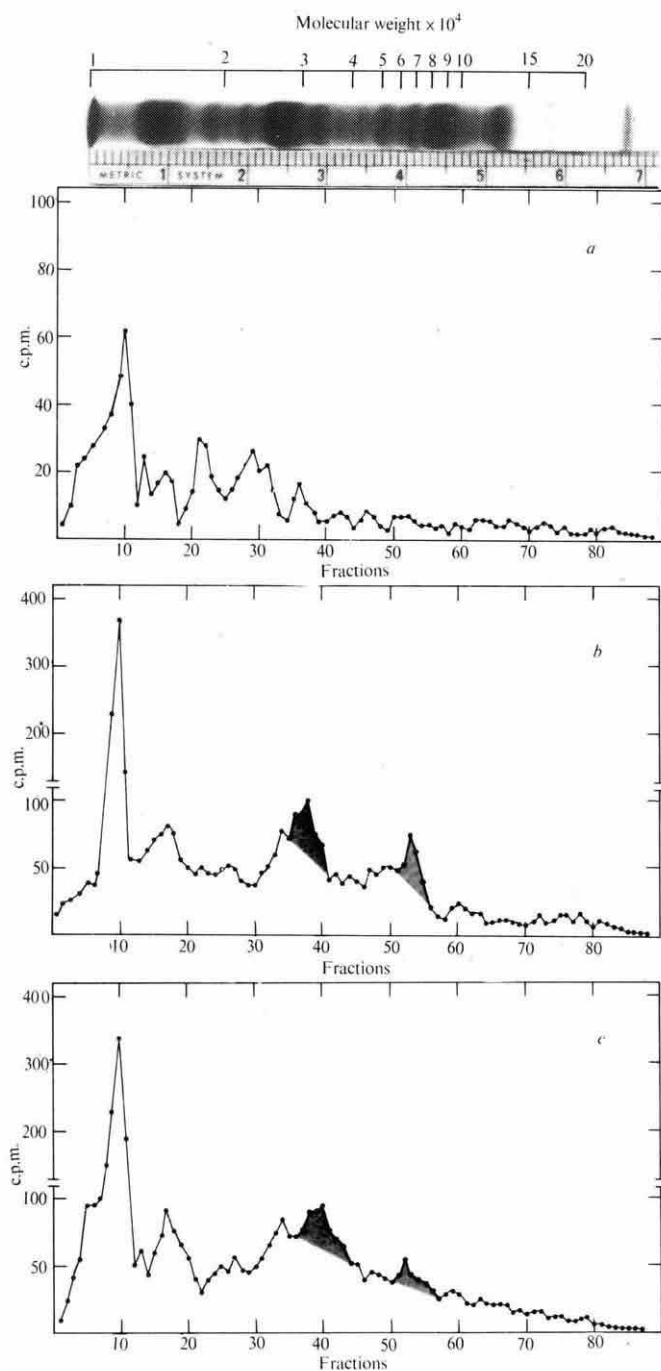


Fig. 4 Comparison of proteins synthesized in actinomycin D-treated and control embryos. Fertilized eggs which were treated with actinomycin D as in Fig. 1 but with a pulse length of 2 h were labelled for 8 h with ^{14}C -valine between stages 12 and 15 (a) and between stages 16 and 19 (b). Both actinomycin D-treated and control embryos were collected separately and supernatant proteins were separated on SDS 10% polyacrylamide gels as in Fig. 3. Actinomycin D-treated ($\circ$) and control embryos ($\bullet$) were compared at the end of stage 15 (a) and stage 19 (b). The shaded area indicates new proteins.

Fig. 3 Polyacrylamide gel electrophoresis of proteins synthesized in the *Fundulus* embryo. Embryos (1,000 per stage) which had reached (a) the high blastula (stage 10) (b) early gastrula (stage 12) and (c) early axiate (stage 16) were labelled with ^{14}C -valine ($1\ \mu\text{Ci ml}^{-1}$, $35\ \text{mCi mmol}^{-1}$) for 8 h. Labelled *Fundulus* embryos were homogenized in a 'Teflon' and glass Potter-Elvehjem tissue homogenizer in 0.25 M sucrose and 0.01 M Tris, pH 7.6, and centrifuged at $105,000g$ for 90 min. The supernatant proteins were placed in 0.1% sodium dodecyl sulphate, 0.01% β -mercaptoethanol, 0.01 M Tris, pH 7.0 (SDS buffer), heated in a boiling water bath and then dialysed overnight against the same buffer. In some cases the supernatant was made pH 11 with 0.1 N NaOH and heated for 15 min at 37°C and then dialysed against SDS buffer. Approximately $150\ \mu\text{g}$ of proteins estimated by the Lowry technique¹⁹ in $150\ \mu\text{l}$ of SDS buffer containing 10% sucrose and 0.001% bromophenol blue was overlaid on 10% polyacrylamide gels, polymerized in glass tubes (10 cm long by 6 mm wide) made according to Laemmli²⁰. The gels were run at 3 mA per gel for 6 h at room temperature. Protein bands were fixed in 50% trichloroacetic acid, stained with 0.002% Coomassie blue and destained in 15% acetic acid. A representative gel is shown above. Molecular weights of proteins were determined according to Weber and Osborn²¹. The radioactive profiles were resolved by cutting gels and collecting approximately 1 mm of gel per scintillation vial. The slices of gel were dissolved in $300\ \mu\text{l}$ of 30% hydrogen peroxide at 55°C for 2 h. The solution was counted in a Bray's type scintillant with a counting efficiency for ^{14}C of 50%. The shaded area indicates new proteins.



development decreases^{6,7,12}. Experience with pactamycin¹² has led to similar results in terms of inhibition of translation although some of the effects are more immediate, while ethidium bromide has no effect on morphogenesis (unpublished results of R. Crawford). Early cytoplasmic proteins not given the correct opportunity to be synthesized before the first cleavage may place certain following protein syntheses out of their proper sequence. A serial order of RNA synthesis and translation of the product shortly after fertilization and well within the one cell stage appears to be essential in the regulation of post-blastula morphogenesis in *Fundulus*.

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by acrylamide gel electrophoresis²; one of these is lysozyme (muramidase)¹. We have reported³ large quantities of lysozyme to be the major cationic protein in the urine of five out of seven cases of chronic myelocytic leukaemia. The specific proteinuria occurred when induction of remission was achieved after X irradiation directed against the enlarged spleen.

We here report the presence of a second basic protein in leukaemic urine that does not cross react antigenically with human lysozyme. This cationic protein has a different electrophoretic mobility from lysozyme, RNase and DNase. Its molecular weight is 23,000 as determined by two-dimensional SDS polyacrylamide gel electrophoresis⁴⁻⁶. It was found to be present in two out of seven cases of chronic myelocytic leukaemia following drug or X-ray therapy, but absent (or present in only negligible amounts) in sixteen cases of acute monocytic leukaemia and three cases of acute myelogenous leukaemia.

The protein could be identified also in leukocyte lysates from chronic myelocytic leukaemics using specific antisera. The protein will therefore be referred to as cationic leukocyte antigen (CLA).

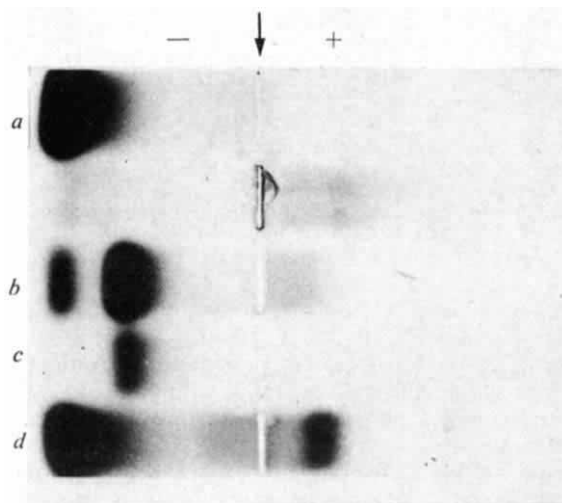


Fig. 1 Electrophoresis in 5% polyacrylamide gel of lysozyme (LZM), cationic leukocyte antigen (CLA), and post γ -globulin. The gel and electrode buffer was 0.06 M ammonium bicarbonate, pH 8.2. Channels contain, a, purified LZM from urine of a monocytic leukaemic; b, bentonite preparation from urine of a chronic myelocytic leukaemic consisting of 80% CLA and 20% LZM; c, isolated and purified CLA; d, mixture of LZM and post γ -globulin.

Twenty-four-hour samples of urine were collected and the lysozyme activity was estimated by the method of Osseman and Lawlor⁷. In case of increased urinary lysozyme activity during X-ray therapy (or cytostatic therapy with mercaptopurine (Purinethol) in blastic crises) cationic proteins were isolated from the urine by the bentonite adsorption and elution method⁸. The isolation procedure was repeated at weekly intervals. Seven out of twelve cases of chronic myelocytic leukaemia had lysozymuria following X-ray or drug treatment. In two cases CLA was the only cationic constituent besides lysozyme demonstrated by polyacrylamide gel electrophoresis of the bentonite preparation (Fig. 1).

A change in the ratio of lysozyme to CLA during the course of the disease was seen in one case in which the patient was tested for a year. An increase from 44% to 51% was observed for CLA after X-ray therapy. An increase to over

Cationic Leukocyte Antigen in Urine of Patients with Chronic Myelocytic Leukaemia

THE cytoplasmic granules of both animal¹ and human² leukocytes contain several basic proteins, some of which show antibacterial activity. Eight or nine such components of granules of human leukaemic myeloid cells can be separated

80% was seen when Purinethol was administered. With prolonged remission the CLA level was markedly reduced. The daily urinary output of CLA was between 17 and 235 mg d⁻¹. CLA may thus serve as an invaluable indicator of the response to therapy in certain kinds of leukaemias (G. L., F. W. T., W. W., and M. M. T., in preparation).

The nature of cationic leukocyte antigen is unclear. Its electrophoretic mobility in two-dimensional SDS gels indicates that it has a higher molecular weight than lysozyme. Using *Escherichia coli* ribosomal proteins of known molecular weights ranging from 9,600 to 65,000 to calibrate the gels⁹, we found that the apparent molecular weight of CLA is about 7,000 daltons greater than that of lysozyme.

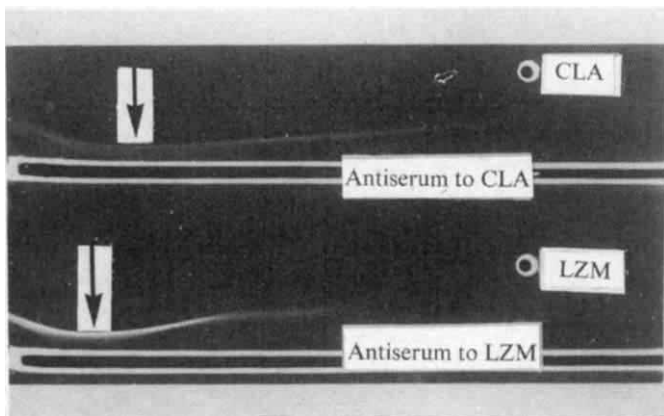


Fig. 2 Immunoelectrophoretic characterisation of human lysozyme (LZM) and cationic leukocyte antigen (CLA). Note the absence of antigenic cross reaction with antiserum to cationic leukocyte antigen. The electrophoretic mobilities of LZM and CLA are indicated by arrows.

CLA was isolated from the urine by elution from 5% polyacrylamide gels (F. W. T., G. L., W. W., G. W. T., M. M. T., and R. Böhm, in preparation). Antisera were prepared by immunising five rabbits with purified CLA using Freund's complete adjuvant. The serum of two rabbits contained antibodies with specificity for CLA. These antisera precipitated only CLA in immunoelectrophoresis (Fig. 2) and Ouchterlony double diffusion experiments as demonstrated by testing with known cationic urinary proteins such as lysozyme and post γ -globulin, and whole normal urine and serum. RNase and DNase gave a negative precipitation reaction. Precipitation-inhibition analysis¹⁰ also failed to demonstrate any cross reaction. The CLA-specific antisera did not inhibit the lytic activity of lysozyme, in contrast to lysozyme-specific antiserum which inhibited lysozyme activity. Purified CLA did not exhibit lytic activity towards the lysozyme substrate, *Micrococcus lysodeikticus*. Tryptic peptide fingerprint patterns of totally reduced and alkylated CLA were strikingly different from those of human lysozyme¹¹.

As renal function was normal in the two patients, the large quantities of CLA in the urine of these patients probably represent a simple threshold phenomenon and are not due to renal tubular dysfunction. Increased elaboration of CLA in patients with chronic myelocytic leukaemia may result from the increased lysosomal breakdown induced by X irradiation and the cytostatic therapy of the granulocytic cells and their precursors. Patients with chronic myelocytic leukaemia may serve as an invaluable source of homogeneous proteins for studies designed to elucidate the structure, function and genetic control of the cationic proteins of human granulocytes.

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Bacteria Swim by Rotating their Flagellar Filaments

It is widely agreed that bacteria swim by moving their flagella, but how this motion is generated remains obscure^{1,2}. A flagellum has a helical filament, a proximal hook, and components at its base associated with the cell wall and the cytoplasmic membrane. If there are several flagella per cell, the filaments tend to form bundles and to move in unison. When viewed by high-speed cinematography, the bundles show a screw-like motion. It is commonly believed that each filament propagates a helical wave³. We will show here that existing evidence favours a model in which each filament rotates.

The idea that the flagellar filaments might rotate relative to the cell body as rigid or semi-rigid helices has appeared intermittently in the literature, but a convincing case for such a model has not been made. Rigid rotation was noted as a possibility by Stocker in 1956⁴. Doetsch argued for it in 1966^{5,6}, but he abandoned the idea in 1969^{2,7} in favour of one in which the flagella "wobble" and only appear to rotate. Recently, Mussill and Jarosch⁸ photographed preparations of *Spirillum volutans* squeezed between slides in which the flagellar bundles remain "motionless" while the wave movement appears in the cell body. They conclude that "the body must rotate around the point of insertion of the flagellar bundle". Rotation of this kind is assumed in a hydro-mechanical analysis of *Spirillum* by Chwang *et al.*⁹. Since the flagellar filaments of *Spirillum* arise separately¹⁰, an alternative possibility is that each filament rotates individually. This alternative, which we favour, makes sense for peritrichously flagellated bacteria as well. The flagellar bundles of these cells cannot rotate as a unit, because their filaments arise at widely distant points on the surface of the cell.

It is possible to envisage a biological rotary motor consistent with electron micrographs of structures at the base of a flagellum. In their model of the basal end of a flagellum

extracted from *Escherichia coli*, DePamphilis and Adler¹¹ show a rod extending from the end of the proximal hook and enclosed by rings of diameter 22.5 nm. Suppose that the ring associated with the cytoplasmic membrane, the M ring¹², is rigidly coupled to the rod yet free to rotate in the membrane, and that the rod is able to rotate freely within the other components through which it passes. Rotation of the flagellum would then be possible if the periphery of the M ring was linked to the cell wall by cross bridges of the kind found in skeletal muscle.

Such a rotary motor would be capable of delivering the power required to drive a flagellum. Coakley and Holwill¹³ estimate the power dissipation of an isolated filament running at 50 Hz to be about 4×10^{-10} erg s⁻¹. If the cross bridges step along the periphery of the M ring with a step length of 8 nm (ref. 14), the ring will rotate once about every nine steps, and the filament can be driven at 50 Hz if each cross bridge steps at a rate of about 450 s⁻¹, which is a reasonable figure¹⁵. Huxley and Simmons¹⁴ judge the external work which can be done per cycle of attachment and detachment of a cross bridge to be about 3×10^{-13} erg. Therefore, three cross bridges stepping at the above rate can generate the necessary power. If there are three cross bridges, the average force which each must exert is about 3.8×10^{-7} dyne, a value only slightly larger than the isometric force exerted by a cross bridge in the experiments of Huxley and Simmons¹⁴. There is room for five cross bridges along the periphery of the M ring, if their spacing is similar to that in skeletal muscle¹⁵.

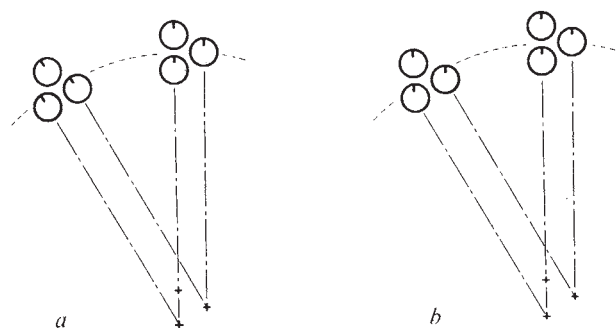


Fig. 1 A schematic cross-sectional view of a bundle of three flagellar filaments rotating individually (a) or propagating helical waves (b) at two successive instants of time. The section is in a plane normal to the helical axes (+), and the motion is in the frame of reference of the body of the cell. Each filament is marked at a fixed point. In (a) the filaments rotate relative to one another; in (b) they do not. The thrusts generated are the same.

Although cross bridges of the kind found in skeletal muscle have not been seen at the basal end of a flagellum, they may have been washed away in the procedure of DePamphilis and Adler¹⁶ or obscured in thin-sectioned material by components of the cell wall. Of course, the construction of the motor may be entirely different. We have made the above calculations only to demonstrate the plausibility of a biological rotary motor. Indeed, there appears to be as much, if not more, structural evidence for this model than for one in which the flagellar filaments propagate helical waves^{1,2}, especially if the energy for the external work which each element of the filament must do is provided by a driving mechanism at the base^{3,17}.

The model in which the filaments rotate individually may not be argued against on the basis of the greater power required to rotate a helical filament than to propagate a helical wave. Coakley and Holwill¹³ find that most of the power dissipation is due to the lateral displacement of the fluid, which is the same in both models. For an isolated

filament the power dissipation for rotation is only 0.5% larger than the power dissipation for wave propagation; for a bundle of 200 filaments separated by aqueous fluid of mean thickness equal to the filament radius, the figure is 50%. The latter increase in dissipation may be unrealistically large, since there is no reason *a priori* to suppose that the filament-to-filament separation is as small as the filament radius. Nevertheless, the dissipation per filament for the bundle is relatively small, because the filaments work together in displacing the fluid laterally. For the bundle of 200 filaments rotating individually and in phase, it is only 1.5% of the dissipation for the isolated filament¹³.

One cannot distinguish the various models by the fact that the body of a freely swimming cell rotates about an axis parallel to the direction of motion in a sense opposite to the rotation (or the apparent rotation) of the flagellar bundle. As discussed by Taylor¹⁸, the torque responsible for the rotation of the body is generated, in the main, by the lateral displacements. This effect is an essential component in more recent theories^{13,19,20}.

One can, however, distinguish the models by experiments sensitive to the rotation of filaments relative to one another. This occurs when the filaments rotate individually (Fig. 1) but not when they propagate helical waves, "wobble"⁷, or rotate as a group⁸. If the filaments rotate individually and two are linked together, it is likely that the entire bundle will stop. The linked filaments will stop, because they can no longer rotate relative to one another; the others will stop, because their lateral motion will be blocked by the first two.

Cross-linking experiments have been done, and the results clearly favour individual rotation. Greenbury and Moore²¹ find that the peritrichous bacterium *Salmonella typhimurium* remains motile when as many as 10^5 univalent flagellar antibodies are adsorbed per cell, a mass of antibody four times that of the mass of the flagella. About half of the cells are immobilized when about 200 bivalent antibodies are adsorbed per cell. They conclude that the bivalent antibody acts by linking the filaments together. An alternative interpretation is that flagellin molecules loaded with univalent antibodies still function, but molecules in the same filament cross linked together do not. This possibility is eliminated in an experiment by DiPierro and Doetsch²², who compare the effect of univalent and bivalent flagellar antibodies on *E. coli*, which is peritrichous, and on *Pseudomonas fluorescens*, which has only one filament. In the presence of univalent flagellar antibodies both kinds of bacteria remain motile. In the presence of bivalent antibodies *E. coli* stops, but *P. fluorescens* remains motile until the cells are linked together. "Very few non-motile single cells were observed in the *Pseudomonas* culture being immobilized, whereas with the *Escherichia* culture, large numbers of single non-motile organisms were observed, and in both cases this condition existed before the appearance of microscopically visible clumps"²².

Results of experiments with bacteriophages that attack flagella provide additional evidence for the individual rotation model. Meynell²³ notes that rapidly swimming *Salmonella abortus-equi* stop abruptly on swimming into a drop of χ phage at high titre, even after the phage has been inactivated by radiation or over-centrifugation. The adsorption of the phage did not cause any morphological changes in the flagella, as far as could be seen by electron microscopy. Raimondo *et al.*²⁴ conclude that the adsorption of one PBS1 phage to one flagellum of *Bacillus subtilis* is sufficient to render the entire complement of flagella non-functional. They obtain the same results with phage ghosts treated with DNase, and conclude that "the functioning of all the flagella of *B. subtilis* is under the control of one 'motor'". These results can be explained if the filaments rotate. If the tail fibres of PBS1 wrap tightly enough around even a single filament, the bundle will jam, since the protruding body of

the phage will not be able to rotate past the adjacent filaments.

The individual rotation model suggests a mechanism by which flagellotropic phages are able to infect cells. The phage χ attaches to the filament with one tail fibre and then travels to the base of the flagellum where it injects its DNA; if the filament is inactive, the phage fails to reach the site of injection²⁵. Assuming that the binding of the phage to the filament is not completely rigid, so that some relative motion is possible, it is conceivable that the phage moves down the filament like a nut on a bolt, the grooves between the helical rows of flagellin molecules serving as the threads. If this notion is correct, the phage will move to the base of the flagellum if the filament is rotating and the "threads" are intact; motion of the cell body is not required. There is a mutant of *Salmonella* which is completely non-motile because its filaments are straight²⁶. The defect is in the flagellin, not in the components at the flagellar base. The flagellin molecules are still arranged in helical rows²⁷. As we would predict, the mutant is fully sensitive to χ (ref. 26). There is a similar mutant of *B. subtilis* which remains sensitive to PBS1 (ref. 28).

Although bundle formation by peritrichously flagellated bacteria is not well understood, the forces responsible are thought to be hydrodynamic. If the viscosity of the medium is increased slightly, for example by the addition of methylcellulose, the bundles scatter more light⁴. The bacteria also swim more rapidly²⁹. We find in tracking experiments³⁰ that they swim less erratically. These effects can all be explained, if, as argued by Stocker⁴, the increased viscous stress causes more filaments to join bundles. If hydrodynamic forces, in fact, cause bundle formation, the observation that the mutant of *Salmonella* possessing straight flagella forms bundles²⁶ provides further evidence for the rotation of the filaments, since in this case interactions due to lateral displacements are absent. We suggest that the bending of the filaments around the sides of the cell is facilitated by the proximal hook, which serves as a flexible coupling or universal joint.

The hydrodynamic basis for the synchronization of filaments within a bundle is more firmly established. Flagella of adjacent spermatozoa³¹ or flagellar bundles of adjacent bacteria⁴ tend to move in phase. Taylor³¹ calculated the forces acting to synchronize parallel undulating sheets and found these forces to be large for sheet spacings smaller than the wavelength of the undulations. Although explicit calculations of this kind have not been made for thin filaments, the work of Coakley and Holwill cited earlier¹³ indicates a large reduction in power dissipation for synchronous rotation within a bundle.

Work by Silverman and Simon³² on "polyhook" mutants of *E. coli* which provides strong support for the rotation model has recently come to our attention. When cells which have abnormally long proximal hooks but no filaments are treated with anti-hook antibody, they form pairs which counter-rotate. This result can be explained if the proximal hooks are driven by rotary motors and the antibodies link a single hook from one cell to one or more hooks from another.

If, as suggested by existing evidence, bacterial flagella rotate, the structures at the base of the flagellum deserve more attention than they have received thus far.

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Insect Epoxide Hydrase Inhibition by Juvenile Hormone Analogues and Metabolic Inhibitors

EPOXIDE rings are known to be enzymatically hydrated by numerous organisms¹⁻³ to give predominantly the corresponding *trans*-dihydroxy compounds and the epoxide hydrases involved can moderate various types of bioactivity. Thus, they are involved in the deactivation of certain labile aromatic epoxides which may be responsible for carcinogenesis² and in the detoxication of insecticides such as 1-naphthyl-N-methylcarbamate (carbaryl) or the cyclodiene dieldrin.

The epoxide ring of dieldrin is only slowly attacked by epoxide hydrases⁴, but that of the analogue HEOM (I, Fig. 1) varies in stability according to the species. Thus, HEOM is toxic to tsetse flies, which have poor hydrative ability, but innocuous to other insects having high epoxide hydrase activity⁵.

For cecropia juvenile hormone (JH; III, Fig. 1) and its mimics containing terminal epoxide rings, hydrative epoxide ring cleavage (and ester hydrolysis, when this function is present) is a significant metabolic deactivation route in several species^{6,7} and the epoxide ring has a variable influence on the activity of analogues containing it that may partly reflect differences in its biodegradability⁸.

A search for inhibitors of the epoxide hydrase which rapidly converts HEOM³ into the corresponding *trans*-diol (II, Fig. 1) in several insects has revealed that synthetic cecropia hormone (mixed isomers), as well as methyl epoxyfarnesoate (MEF) and the synthetic mimics EPGE⁹ (Table 1) and EGSE¹⁰ (V, Fig. 1), are competitive inhibitors of cyclodiene epoxide hydrase in pupal homogenates of the blowfly, *Calliphora erythrocephala*, and the mealworm, *Tenebrio molitor*, indicating that these compounds are possibly alternative substrates for HEOM-hydase. In

this case, inhibitors of the cyclodiene hydase may also be expected to inhibit JH-epoxide hydase, and the inhibition of enzymatic HEOM-*trans*-diol formation (detectable by electron-capture gas-chromatography¹¹) affords a useful preliminary screening method for epoxide hydase inhibitors.

Juvenile hormone analogues may either mimic the action of the natural hormone at the sensitive site(s), or, by inhibiting deactivation enzymes, merely stabilize the levels of endogenous hormone at times damaging for the insect. It is probable that both mechanisms operate in some cases. That hormone mimics such as piperonyl butoxide, sesoxane and the terpenoid compounds of Table 1 are inhibitors of the *Tenebrio* pupal HEOM-hydase indicates that the large amounts of these compounds normally applied in testing for hormonal activity, relative to the levels of endogenous hormones, will have a significant effect on the enzymatic hydration of any hormone epoxide rings present, apart from effects on other hormone metabolizing enzymes such as double bond hydases, microsomal oxidases and possibly esterases.

Accordingly EGSE (10^{-4} M; fifteen-fold excess over added substrate) markedly inhibits (90%) the degradation of the hormone analogue IV (Fig. 1; detectable by GC and electron-capture) by homogenates (Table 1) of *T. molitor* pupae and EPGE is somewhat less effective (38% inhibition at 3×10^{-4} M). These homogenates hydrate HEOM and decompose IV and the conversions are unaltered in either aerated or anaerobic buffer, indicating the absence of significant microsomal oxidase or nitro-reductase activity in these conditions (no added cofactors), which are optimal for HEOM-hydase. Piperonyl butoxide is a moderate inhibitor of HEOM-hydase (Table 1), but at 3×10^{-4} M did not stabilize IV, and is 10^3 to 10^4 times less effective as a hormone mimic than either EGSE or EPGE *in vivo* in *T. molitor*^{9,10,12}. Sesoxane also had no effect at this concentration *in vitro*, although it is rather active toward this insect *in vivo*.

The effects of metabolic inhibitors will clearly depend on both the species examined and the hormone substrate.

Thus, piperonyl butoxide, as well as the 3-ethyl-analogue of EGSE¹⁰ and other hormone analogues containing ester and amide groups, stabilize JH itself up to nineteen-fold in midgut homogenates from the southern armyworm, *Prodenia eridania*, and the natural hormone of this lepidopteran is likely to be JH or a similar compound¹³.

Of numerous compounds examined, simple epoxides seem to be the best inhibitors of HEOM-hydase (Table 1). The most effective of these, 1,1,1-trichloropropane-2,3-epoxide (ETP) and 1,2,3,4-tetrahydronaphthalene-1,2-epoxide, inhibit the enzyme non-competitively, in contrast to the competitive inhibition found with other epoxides such as JH. Some, like 1-(4-propargyloxyphenoxy)-2,3-epoxypropane (PPPGE) or 1-(3,4-methylenedioxyphenoxy)-2,3-epoxypropane¹⁴ (SESGE; Table 1), safrole epoxide and isosafrole epoxide, inhibit microsomal oxidases as well as epoxide hydases, as shown by their synergistic action with carbaryl against houseflies and blowflies. ETP, PPPGE and the bis-epoxide BDGE (Table 1) inhibit the degradation of the hormone analogue IV (Fig. 1) in homogenates of *Tenebrio* pupae, and the last two compounds have also been shown to stabilize JH in the midgut preparation from southern armyworm (Slade, M., and Wilkinson, C. F., personal communication).

The mechanism of enzymatic epoxide ring hydration is still obscure, but activity is optimal at alkaline pH and, chemically, the nucleophilic attack of OH⁻ on an oxirane carbon may be likened to its attack on the carbonyl group of a carboxylic ester or on the phosphorus atom of an organophosphate during the alkaline hydrolysis of these chemicals. It is therefore interesting that a non-toxic carboxyesterase inhibitor, such as triphenyl phosphate, inhibits blowfly HEOM-hydase competitively and that the microsomal oxidase inhibitor SKF 525A (Table 1), which contains a carboxylic ester function, is also an inhibitor of HEOM-hydase *in vitro*⁵. Such compounds are potential stabilizers of both of the terminal functional groups in ecropia hormone.

Simple inhibitors of microsomal oxidases, epoxide

Table 1 Effect of Various Compounds on the Cyclodiene Epoxide Hydase in Pupal Homogenates* of the Blowfly, *Calliphora erythrocephala*, and Mealworm, *Tenebrio molitor*

Compound	Homogenate source			
	<i>C. erythrocephala</i>		<i>T. molitor</i>	
	% Inhibition	[I]/[S] †	% Inhibition	[I]/[S] †
Hormone types:				
Cecropia hormone (JH) (mixed isomers)	40	4	34	4
Methyl <i>trans</i> , <i>trans</i> -10,11-epoxy farnesoate (MEF)	37	4	23	4
1-(4-ethylphenoxy)-6,7-epoxy-3,7-dimethyl-2-octene (EPGE)	44	4	60	4
6,7-epoxy-3,7-dimethyl-1-(3,4-methylenedioxyphenoxy)-2-cis/trans-nonene (EGSE)	64	4	60	4
Microsomal oxidase inhibitors:				
Piperonyl butoxide	64	10	54	10
2-(3,4-methylenedioxyphenoxy) 3,6,9-trioxaundecane (sesoxane)	30	10	50	10
2,3-methylenedioxy-naphthalene	50	10	30	10
2-diethylaminoethyl 2,2-diphenyl valerate hydrochloride (SKF 525A)	74	10	68	10
Non-hormone epoxides:				
1,1,1-trichloropropane-2,3-epoxide (ETP)	63	0.04	33	0.2
1,2,3,4-tetrahydronaphthalene-1,2-epoxide (ETN)	50	0.12	100	0.5
1-(1-naphthylloxy)-2,3-epoxy-propane	69	1	47	1
1-(4-propargyloxyphenoxy)-2,3-epoxypropane (PPPGE)	52	0.25	47	1
1-(3,4-methylenedioxyphenoxy)-2,3-epoxypropane (SESGE)	50	0.5	43	1
2,2-bis[4-(2,3-epoxypropyloxy)phenyl]propane (BDGE)	50	0.3	100	1
Organophosphorus compounds:				
Triphenyl phosphate	95	10	88	10
Diethyl phenyl phosphorothionate	66	10	88	10
S,S,S-tributyl phosphorotrithioate	78	20	40	10
Propyl 2-propynyl phenylphosphonate	29	10	11	2

* 10% (w/v) homogenates of fresh pupae in 0.1 M phosphate buffer, pH 8.5. For incubation (static for 12 min at 30° C), substrate (100 µg HEOM, final concentration 5.5×10^{-5} M) and inhibitor, each in 50 µl ethanol, added to same buffer (4.5 ml) plus homogenate (0.5 ml).

† Inhibitor concentration/substrate concentration.

hydases and carboxyesterases may possibly be used as synergists for either biodegradable organochlorine compounds such as HEOM, or labile hormone analogues. Metabolic deactivation is a likely cause of cross resistance to exogenous JH-analogues in strains of flour beetles¹⁵ and houseflies¹⁶ already resistant to other control agents and synergists may also prove useful in this context.

Several of the epoxides examined here, including EGSE, elicit transient signs of poisoning and knock-down in houseflies, blowflies and fleshflies (*Sarcophaga barbata*), when topically applied before normally innocuous doses of HEOM. Similar effects, seen with SKF 525A or n-propyl 2-propynyl phenylphosphonate, are accompanied by a temporary elevation in the tissue levels of HEOM. Inhibition of the rather active HEOM-hydase in these insects is evidently only transient and the inhibitors are likely to be more effective as synergists in insects such as the stable fly (*Stomoxys calcitrans*) or tsetse fly which have intermediate or poor HEOM-hydase activity respectively.

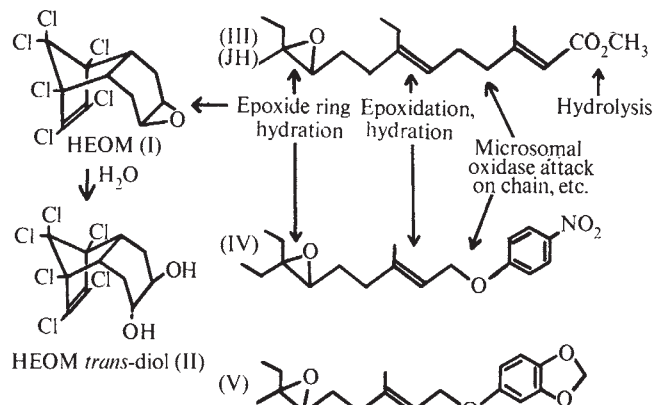


Fig. 1 Metabolic routes for a cyclodiene epoxide, cecropia juvenile hormone and terpenoid hormone mimics.

The non-hormone structural types of Table 1 showed zero or slight morphogenetic activity at the 10 μ g level on topical application to fresh pupae of *Tenebrio* or *Calliphora* but there was some evidence of synergism when certain of them were applied with JH or compound IV (Fig. 1) in the *Tenebrio* test, or with EPGE on *Calliphora* pupae. Also, the bis-epoxide BDGE (Table 1) has shown synergistic effects when co-administered with JH to fifth instar locusts, *Schistocerca gregaria* (Kelly, J. E. N., personal communication). The synergist/hormone ratio for optimal activity may be more critical than for synergist mixtures with classical insecticides. Another problem is that of simultaneously inhibiting metabolic routes catalysed by different enzymes; an inhibitor such as triphenyl phosphate may suppress JH-esterases, epoxide hydases and possibly double bond hydases, but the blockade of these routes to polar derivatives may simply provide extra lipophilic parent substrate for attack by microsomal oxidases, which it does not inhibit. This emphasizes the need for multi-functional inhibitors.

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Delayed Effect of Light on the Mosquito "Clock"

PREVIOUS work on the circadian rhythm of flight activity in the mosquito *Anopheles (Cellia) gambiae* Giles has shown that light can affect flight activity either directly or by changing the phase of the circadian rhythm¹. Neither of these effects seems to account for the fact that, after a change from LD 12:12 (alternating 12 h light : 12 h dark) to DD (constant dark), the first cycle in DD is 1–1.5 h longer than that of subsequent cycles. The results reported here indicate that this increase in period is the result of a delayed effect of light, which is increased if the light period is prolonged, although the evidence suggests that the circadian "clock" is stopped by light. The mosquitoes used were sugar-fed females of *A. gambiae* species A, from a culture originating in Pala, Upper Volta; similar results have been obtained in preliminary experiments with *A. merus* and *A. stephensi*. The flight activity of individual mosquitoes was recorded using the flight sound as an indicator^{1,2}. The insects were reared in an LD 12:12 regime and the temperature throughout was 25°C.

After a change in the light regime from LD 12:12 (50 lux) to DD, with light off at the normal time, the period of the first cycle in DD is about 24 h (mean=24.1 h, s.d.=0.6 h, $n=42$). The rhythm then seems to free-run with a period of between 22.5 and 23 h. When the final light period is prolonged to 18, 24, 36 or 48 h before the regime is changed to constant dark, the light inhibits activity, although with this culture, there is a peak of activity in the light (about 25 h after the previous light off peak) before the activity is damped out. Cyclical activity restarts in phase with the change to constant dark and the first peak of activity follows light off. When the light period is prolonged, however, the period of the first cycle in DD seems to increase with the length of the final light period until, after 48 h light, it is approximately 1 h longer than after 12 h light (mean=25.0 h, s.d.=0.7 h, $n=29$). The period of subsequent cycles in DD does not seem to be affected.

The distortion of the first cycle in DD seems to depend on the light intensity or possibly the quantity of light given in the previous cycle. After a change from LD 12:12 (2 lux) to DD, the mean period of the first cycle in DD was 23.6 h (s.d.=0.7 h, $n=41$) and after the final light period was extended to 48 h it was 24.2 h (s.d.=0.7 h, $n=23$). The

rhythm is still entrained by an LD 12:12 regime when the light intensity is as low as 0.2 lux, but it seems to free-run as soon as it is released into DD (mean period of first cycle = 22.1 h, s.d. = 0.9 h, $n=23$). At this intensity a light period of 48 h does not reset the cycle, which continues in the light with a period of about 24 h.

Light seems to be most effective when it is given late in the subjective day. A light period of 1 h (50 lux) beginning at the normal time of light on increases the period of the next cycle by about 0.5 h, even though it causes a small phase advance in the cycle in which it is given. A similar 1 h light period ending at the normal time of light off increases the period of the next cycle by about 1 h.

The damping out of the cycle when the light period is prolonged suggests that the "clock" is stopped by the light once it extends past a critical phase of the cycle, and in this respect is similar to the "clocks" controlling eclosion in *Drosophila*³ and *Antheraea*⁴. The persistence of an activity peak in the light, before the cycle is damped out, suggests the involvement of a two oscillator system similar to that proposed for the *Drosophila* eclosion rhythm⁵. In this case the A oscillator would be stopped by the light while the B oscillator could continue to the next activity peak. It is possible that the delayed effect of light, described here, could be explained in terms of the interaction of two or more oscillators⁵, but an alternative hypothesis is that light causes the accumulation of some factor which can only affect the period of the rhythm at one particular phase, presumably early in the cycle. The greater effectiveness of light given late in the previous cycle indicates that this factor is short lived.

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Mercury-Selenium Correlations in Marine Mammals

HIGH levels of mercury may occur in the tissues of seals, dolphins and porpoises¹⁻⁴. In the Netherlands from 225 to 765 p.p.m. (wet weight) in liver and from 9.9 to 31 p.p.m. in brain have been detected in adult specimens of the common seal (*Phoca vitulina*) which were found dead⁴. Brain concentrations were of the same order as those in the brain tissue of animals of various species poisoned experimentally by methylmercury compounds⁵. As most of the mercury in marine fish is present as methylmercury⁵, the high levels in seal brain suggest that these animals are affected by the toxic action of methylmercury compounds. Analyses of the methylmercury levels according to Westöb⁶ in the livers of six seals shot in the German part of the Wadden Sea and two adults found dead showed that only 2 to 14% of the total mercury was methylmercury. Similarly, of the 10 and 15 p.p.m. total mer-

cury in the brain of the latter two seals, only 2.2 and 13% respectively could be recovered as methylmercury. Neutron activation analysis of the tissue precipitate remaining after benzene extraction of the acidified homogenates obtained in the methylmercury analysis has shown that on average more than 90% of the mercury remains attached to the tissue components. In humans and in experimental animals poisoned by methylmercury compounds most or probably all mercury is in the form of methylmercury (refs 7 and 8 and S. Skerfving, report by the Department of Environmental Hygiene, Karolinska Institute, Stockholm). Thus the mechanism of binding of mercury to the tissues of seals may be different.

Cadmium, arsenic, selenium, zinc and antimony have been measured in seal tissues by neutron activation analysis⁹. Selenium was present in relatively high concentrations, from 46 to 134 p.p.m. (wet weight basis) in the livers of five seals in which the mercury levels varied from 257 to 326 p.p.m.

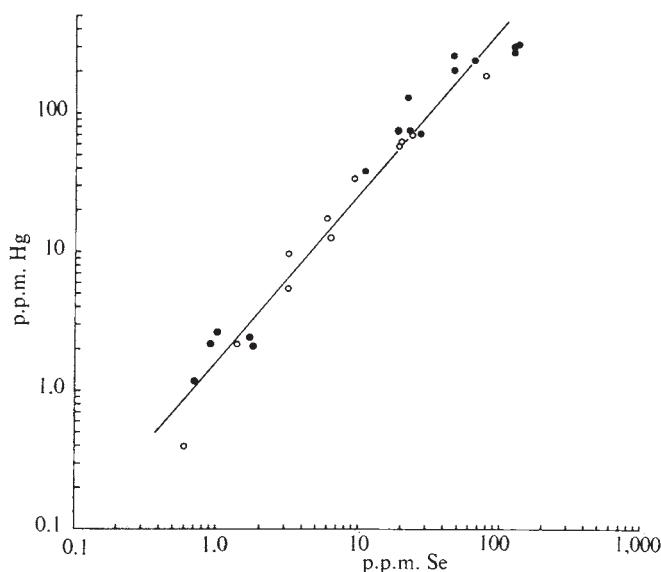


Fig. 1 Correlation between mercury and selenium in the livers of marine mammals. ○, Dolphins and porpoises; ●, common seal.

The range for cadmium was 0.05 to 0.30 p.p.m., for arsenic 0.20 to 1.7 p.p.m. and for zinc 25 to 34 p.p.m.; on the other hand the concentration of antimony was found to be lower than the detection limit of 0.01 p.p.m.⁴. Further analyses were made of juvenile seals found dead, a number of adult seals shot, three porpoises (*Phocoena phocoena*) found dead along the Dutch coast, three bottle-nosed dolphins (*Tursiops truncatus*) which died in a dolphinarium, three dolphins (two *Delphinus delphis* and one *Lagenorhynchus obscurus*) collected near New Zealand and two dolphins (*Sotalia guianensis*) captured in Surinam. The concentration of mercury in the livers of marine mammals seems to be strongly correlated with the concentration of selenium, but not with any of the other elements determined (Fig. 1).

The correlation coefficient between mercury and selenium for the whole group of animals is 0.932, based on a double linear equation. As subgrouping of the animals in their respective taxonomic groups (seals, dolphins and porpoises) did not result in higher correlation coefficients, all three groups may be treated here as one group.

Least squares treatment of the data leads to a regression of mercury on selenium

$$(\text{Hg}) \text{ p.p.m.} = 2.53 (\text{Se}) \text{ p.p.m.} + 17.6 \text{ p.p.m.} \quad (1)$$

Table 1 Distribution of Hg and Se in Subcellular Fraction of Seal Liver

Fraction	Hg (p.p.m.)	Hg (μg)	%	Se (p.p.m.)	Se (μg)	%	Hg/Se
Homogenate	183	3,026		57.2	897		3.4
10% homogenate *	17.7	2,825	100	5.95	1,001	100	2.8
Fraction (I) 10 min 700g	153	1,566	55.4	56.6	578	57.7	2.7
Fraction (II) 10 min 5,000g	30.8	247	8.7	9.38	79.3	7.9	3.1
Fraction (III) 10 min 10,000g	1.56	14.1	0.5	0.94	9.0	0.9	1.6
Fraction (IV) 60 min 105,000g	15.1	141	5.0	4.74	46.8	4.7	3.0
Supernatant IV	0.45	109	3.9	0.17	43.0	4.3	2.5
			73.5			75.5	

* 10% in 0.25 M sucrose solution.

and to a regression of selenium on mercury

$$(\text{Se}) \text{ p.p.m.} = 0.343 (\text{Hg}) \text{ p.p.m.} - 2.0 \text{ p.p.m.} \quad (2)$$

Rearranging equation (2) gives

$$(\text{Hg}) \text{ p.p.m.} = 2.91 (\text{Se}) \text{ p.p.m.} + 5.8 \text{ p.p.m.} \quad (3)$$

The average value of the slopes of equations (1) and (3) is 2.7 ± 0.2 or 1.1 ± 0.1 on a molecular basis. As the mercury and selenium values cover almost 2.5 decades, the highest data pairs have the most profound effect on the form of the equations. Thus, the value of 1.1 ± 0.1 denotes the average uptake ratio of the two elements in the adult stage. Work now in progress indicates a similar correlation for the brain.

Table 2 Mercury-Selenium Ratios in Marine Fish

Sample description	Ref.	Ratio p.p.m. Hg : p.p.m. Se for flesh or total fish	
		Average	Standard deviation
Various kinds of sole, Pacific Coast, United States (16)	11	0.055	0.045
Herring, North Sea (7)	12	0.065	0.018
Herring, North Sea (5)	†	0.05	
Mackerel, North Sea (2)	12	0.04	
Mackerel, North Sea (5)	†	0.02	

No. of samples is shown in parentheses.

† J. H. K., C. H. M. K.-H., P. S. T. and J. J. M. de G., unpublished results.

We have made neutron activation analyses of various subcellular fractions of a fresh liver homogenate from a seal shot in a poor condition (Table 1). More than 50% of the mercury and selenium occurs in the 700g fraction (chiefly nuclei and cell wall material) in which the (weight) ratio is 2.7, again corresponding to a 1:1 molar ratio. The other fractions show the same tendency, assuming a larger analytical error at lower mercury and selenium levels. A similar 1:1 molar increment has been reported for tuna by Ganther¹⁰. Various fish species which marine mammals feed on contain, however, at least forty times more selenium than mercury (refs 11 and 12 and unpublished results of J. H. K., C. H. M. K.-H., P. S. T. and J. J. M. de G.) (Table 2) indicating that the 1:1 molar ratio reported here is established in the aquatic mammal itself.

Our results may suggest that the correlation found reflects a causal relationship between mercury and selenium in marine mammals. As selenium has a protective effect against the toxic action of mercury compounds in experiments with rats and Japanese quail^{10,13-15} it may have a similar effect in marine mammals. It has been suggested that mercury and selenium occur together in animal tissues and are associated to proteins by means of sulphur¹⁴. Our observation that most of the mercury in seal liver and brain was tightly bound and

could not be recovered in the form of methylmercury may support this suggestion.

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Induction of Liver Tumours in Rats by a Single Treatment with Nitroso Compounds given after Partial Hepatectomy

AN outstanding problem in understanding the mechanism of carcinogenesis by nitroso compounds is the apparent immunity of the liver to a single treatment with the carcinogen. While dimethylnitrosamine (DMN) in the diet induces tumours in liver and kidney of rats, a single injection given to adult animals usually induces only kidney tumours¹. One treatment

with nitrosomethylurea (NMU) induces tumours in many tissues, but not in liver^{2,3}. In each case, liver cells have been transformed by treatment *in vitro* with the carcinogen⁴. Therefore the question arises of why one administration of a carcinogenic methyl-nitroso compound, or of most other liver carcinogens, rarely induces liver cancer in the intact adult rat. One answer is suggested by experiments in which a high incidence of liver cell cancer could be induced by one treatment with a nitrosamine if given after partial hepatectomy.

Surgical removal of 65% of the liver causes a wave of mitoses in hepatocytes of the liver remnant⁵. A single treatment with DMN during this period of restorative hyperplasia induced a 40–60% incidence of liver cell cancer⁶. The experiments described here show that one injection of DMN or of diethylnitrosamine (DEN) given after partial hepatectomy resulted in a high incidence of hepatocellular carcinoma. By using younger animals, the time before death due to liver cancer in animals given DMN 24 h after the operation was reduced from 2 to ~1.5 yr.

Animals were partially hepatectomized by the method of Higgins and Anderson⁷, and dialkyl nitrosamines were administered as shown in Table 1. (The details of these experiments are to be submitted for publication elsewhere.) The animals were kept until they died or until they appeared ill, when they were killed, and relevant tissues examined histologically. A single treatment of intact animals with DMN did not induce liver tumours. When DMN was administered after partial hepatectomy, a high proportion of the animals developed nodular livers, the nodules often having an abnormal lobular structure and evidence of liver cell damage. Hyperplastic areas and biliary cystadenomas were also present. Some animals had liver cell carcinoma. This was usually trabecular carcinoma as seen previously⁶, and in some instances metastases

were found in the lung. Borderline cases which were difficult to classify were not recorded as hepatocellular carcinoma. A higher incidence of tumours was obtained if the DMN was given at 24 h rather than 0–2 h after the operation.

The dependence of vulnerability on the time of administration of DMN after the operation may be due to variation in the sensitivity of the cells at different stages in the cell cycle, with the 24 h stage, at which DNA synthesis is at a maximum, being the most vulnerable. Cell cycle dependence would also be affected by the time taken to metabolize the carcinogen. In the case of DMN, a low dose given soon after the operation would be completely metabolized by 24 h⁶. Therefore it is possible to select the time at which the damage caused by DMN metabolism occurs, by appropriate administration at 0–2 h or at 24 h. In experiments with DEN, a considerably higher dose of nitrosamine was used, and it is possible that DEN given 0–2 h after partial hepatectomy was still being metabolized 24 h later. Thus, the results of experiments with DMN are the more relevant when considering the cell cycle dependence of carcinogenesis.

A similar result was obtained with N-methyl-N'-nitro-N-nitrosoguanidine (MNNG). A single treatment given to an intact animal induces biliary cystadenoma⁸, but when given after partial hepatectomy it can induce liver cell cancer (Table 1). NMU administered after partial hepatectomy has not so far induced liver cancer. This may be due to the fact that the liver tumours induced by the nitrosamines and by MNNG took about 1.5–2 yr to develop, while most animals treated with NMU died before this time had elapsed from more rapidly growing tumours. NMU breaks down spontaneously without the need for metabolic activation, and as its effects do not depend on the tissue distribution of activating enzymes they are widespread. Hence most animals die of the fastest

Table 1 Effect of Partial Hepatectomy (PH) on Response of Rats to Treatment with Biological Alkylating Agents given in Saline at the Time Stated

Alkylating agent	Sex and body wt	Time of treatment after PH (h)	Dose (mg kg ⁻¹)	Total	Nos. of animals Acute death	Survivors	Hepatocellular carcinoma among survivors
DMN	♂ 100 g	No PH	4.8	6	0	6	0/6
		0–2	4.8–7.2	11	5	6	1/6
			8.4	6	6	0	
		24	4.8	6	0	6	2/6
	♀ 100 g	No PH	8.4–12.0	10	0	10	0/10
		0–2	6.0–7.0	4	0	4	1/4
			8.4	7	2	5	0/5
			9.6–13.2	7	4	3	0/3
		24	9.6	1	0	1	0/1
DEN	♀ 100 g	No PH	100–130	10	1	9	2/9
		0–2	50–114	9	0	9	4/9
			130–300	15	15	0	
		24	130	6	1	5	1/5
			150–200	5	5	0	
	♀ 100 g	No PH	10+10	4	0	4	0/4
			15+10	6	0	6	0/6
		0–2 and 3–5	10	3	0	3	0/3
			15	1	1	0	
			10+10	7	2	5	2/5
MNNG*	♀ 100 g	No PH	15+10	4	2	2	0/2
	♀ 200 g	No PH	66	2	0	2	0/2
			88	4	0	4	0/4
		0–2	22	1	0	1	0/1
			44	2	0	2	0/2
			66	8	0	8	0/8
MMS	♀ 200 g	No PH	88	7	3	4	0/4
			110.5	6	5	1	0/1
		24	66	3	0	3	0/3
			88	8	1	7	0/7
	♀ 200 g	No PH	66	2	0	2	0/2
			88	4	0	4	0/4
		0–2	22	1	0	1	0/1
			44	2	0	2	0/2
			66	8	0	8	0/8

DMN, DEN and MMS were given by intraperitoneal injection. MNNG was administered by stomach tube, the dose being split as shown.

* Dose expressed as mg per rat.

developing tumours, especially lymphomas. This early death does not occur after treatment with nitrosamines as metabolic activation is required before these compounds are biologically effective and so, because they are metabolized chiefly by liver and kidney in the rat, it is usually only in these organs that tumours are induced.

There is much evidence to suggest that the nitrosamines, MNNG and NMU are carcinogenic because of their ability to alkylate macromolecules^{9,10}, and therefore the question arises as to why other biological alkylating agents are not also potent carcinogens. Methyl methanesulphonate (MMS), for example, was found not to induce liver cancer even when given after partial hepatectomy (Table 1). But, while nitrosamines, MNNG and NMU, apparently react with nucleic acids by a predominantly SN1-type mechanism, forming O⁶-methylguanine among the reaction products, MMS reacts predominantly by an SN2-type mechanism^{9,10}. O⁶-Methylguanine is formed in liver DNA of partially hepatectomized rats treated with DMN, but none was detected in animals treated with MMS¹¹. This difference in the pattern of DNA alkylation may well be relevant in carcinogenesis¹².

These experiments suggest that for carcinogenesis to result from treatment with alkylating agents, two factors are necessary. One is damage of a special type to the genetic material. The second is the occurrence of cell replication before the damage has been repaired. Cell division may be necessary so that mispairing of the alkylated bases may take place during DNA replication, and thereby convert the transitory abnormality of aberrant methylation into an inheritable change in base sequence. There is evidence that the presence of O⁶-methylguanine in the template can cause aberrant base-pairing¹³.

The concept that both chromosome injury and mitosis are necessary for carcinogenesis is not new, but it has been considered chiefly in relation to the induction of skin cancer. In this case, chemical injury is believed to initiate a process which is promoted by agents which cause hyperplasia¹⁴. The idea that a similar two-stage process may be relevant in carcinogenesis of liver has been postulated by Maini and Stich¹⁵ and by Warwick¹⁶ and others^{17,18}. The experiments with nitroso compounds described above support the hypothesis that genetic damage may need to be followed by cell replication in order to convert a transient into an inheritable change, such as presumably must occur during carcinogenesis.

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Anaerobiosis in the Overwintering Beetle *Pelophila borealis*

At Finse, situated at 1,200 m above sea level in the mountain plateau of Hardangervidda, Norway, adult *Pelophila borealis* (Coleoptera, Carabidae) overwinter in the soil of a sedimentation flat¹. During the winter the flat is periodically flooded by the adjacent river, and consequently covered by a layer of ice. Sixty to 120 cm of snow covers the ground from late September until early June. Soil temperatures at the beetle's overwintering sites are normally slightly below 0° C, but rarely drop below -3° to -4° C.

In these conditions the beetles are frequently enclosed by ice for several months. Their survival seems to depend not only on a sufficient degree of cold-hardiness¹, but also on their ability to tolerate an oxygen deficiency during ice enclosure. For insects overwintering in habitats of this kind, anaerobic metabolism may be an important aspect of winter survival.

Anaerobiosis is known to occur in a number of insects, and many species can survive several hours of anoxia². Some soil-inhabiting insect larvae tolerate 2 or 3 weeks in CO₂³. Several end products of anaerobic metabolism are known from insects, the main ones being pyruvate, lactate and α -glycerophosphate^{4,5}. Metabolism of α -glycerophosphate to glycerol may be a further pathway during anaerobiosis⁵. Considerable amounts of alanine may also be accumulated, as shown in *Musca* by Price⁶.

Adult *P. borealis* were collected in the autumn, and stored at their overwintering sites in perforated plastic boxes filled with soil. Their location was marked with a high stick, from which strings led to the boxes. From these sites the beetles could be dug up at any time during the winter. To test their ability to survive under anoxia, the beetles were placed in glass tubes filled with 99.9% nitrogen, and stored at 0° C for various time intervals. The tubes, which were 0.8 cm in diameter and 30 cm long, were heated and pulled out at one end. From six to eight beetles were placed in each tube, which was then pulled out at the other end, filled with nitrogen, and rapidly sealed by melting both the pointed ends.

Following removal from anoxia, survival rates of the beetles were recorded after 1 or 2 d at room temperature. Beetles which seemed to be moving normally were counted as alive, beetles moving but unable to walk as moribund, and those not showing movement within 3 d as dead. The survival of beetles kept from 8 to 127 d in nitrogen at 0° C was almost 100% (Table 1). The tests were carried out with beetles dug up at various times, but no indication

Table 1 Survival of Beetles Stored in Nitrogen at 0° C for Various Times

Storage time (d)	No. of beetles	
	Alive	Dead or moribund
8	18	0
12	15	0
16	8	1
26	14	0
35	25	1
49	34	4
58	10	0
75	11	1
86	7	0
127	8	0

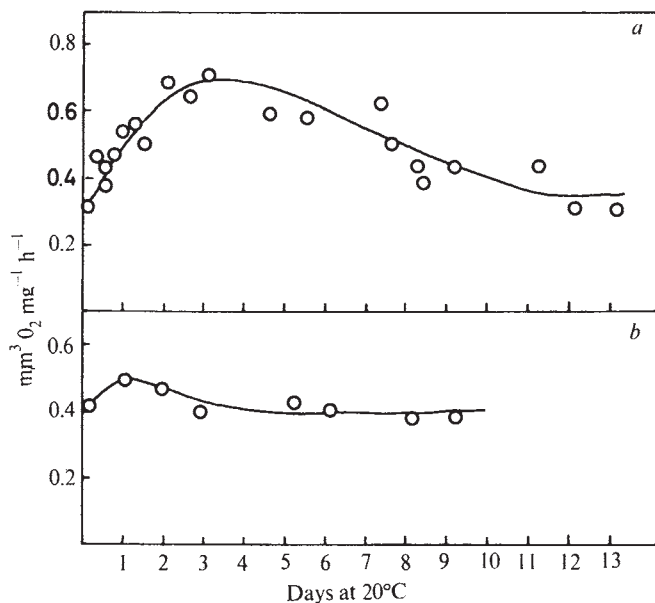


Fig. 1 Oxygen consumption at 20°C in *P. borealis* previously stored in, a, anaerobic and, b, aerobic conditions at 0°C. After removal from 0°C all beetles were kept aerobically at 20°C. Each point represents the average of nine beetles.

of seasonal differences in the ability to survive anoxia from October to March was found.

Oxygen consumption was studied in beetles removed from anoxia after 49 d at 0°C. Measurements were made at 20°C with the constant pressure respirometer described by Engelmann⁷. The first measurements were made a few hours after removal, and then at intervals of 24 h for several days. Between measurements the beetles were kept aerobically at 20°C. No food was supplied. Similarly, beetles stored aerobically in the laboratory at 0°C for about 50 d were transferred to 20°C, and their respiration measured over several days.

Compared with beetles stored aerobically at 0°C, beetles kept anaerobically showed an increased level of oxygen consumption that lasted for several days (Fig. 1). A similar "oxygen debt" has been found in other insects kept in anoxia², and is caused by the oxidation of metabolites formed during anaerobiosis. Similar results were obtained from beetles dug up from their overwintering sites. Flood-

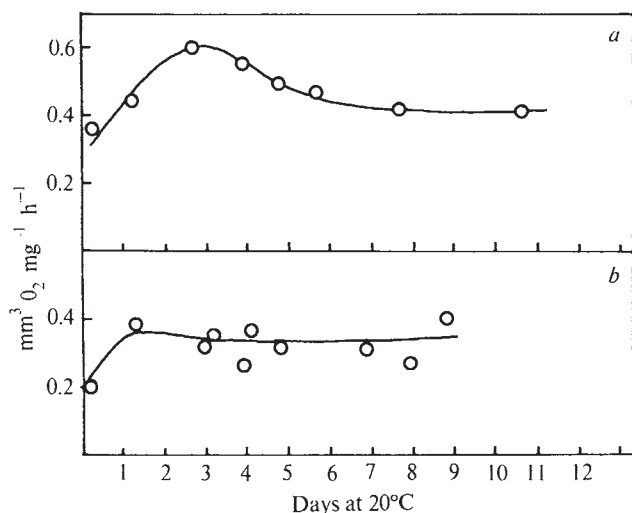


Fig. 2 Oxygen consumption at 20°C in *P. borealis* removed from their overwintering sites in, a, March 1973 and, b, December 1972. After removal the beetles were kept aerobically at 20°C. Each point represents the average of nine beetles.

ing of the sedimentation flat, caused by ice damming the adjacent river, occurred in early February, leaving the beetles enclosed by ice. A normal level of oxygen consumption was found in beetles dug up in December before the flooding but those collected in March showed a considerable oxygen debt (Fig. 2).

Lactate analyses were carried out⁸ on haemolymph samples, collected with a micropipette from a puncture in the neck, from beetles stored in nitrogen at 0°C and those dug up from their overwintering sites. Increasing concentrations of lactate were found in the haemolymph of the beetles with increasing storage time in nitrogen at 0°C (Fig. 3). After about 40 to 60 d the concentration levelled off, indicating that other products of anaerobic metabolism are formed as well. Accumulation of lactate also took place in outdoor conditions. The haemolymph of ten beetles dug up in March contained from 8.7 to 22.7 mM lactate, with an average of 13.8 mM.

To find out if other metabolites accumulate during anaerobiosis, analyses of pyruvate, α -glycerophosphate, glycerol and alanine were carried out in beetles stored for 45 to 70 d in nitrogen at 0°C. For the analyses of pyruvate, α -glycerophosphate and glycerol, extracts of individual beetles were prepared as described by Sømme⁹. Pyruvate was determined according to Sloneker and Orentas¹⁰, α -glycerophosphate according to Heslop *et al.*¹¹, and glycerol according to Sømme⁹. From Table 2 it seems that no difference in contents of pyruvate and α -glycerophosphate could be detected in beetles kept aerobically and anaerobically. No glycerol was accumulated during anaerobiosis, and only traces of this substance were found in both groups.

For analysis of alanine, 10 μl samples of haemolymph were obtained from two or three beetles. The samples were diluted five times with water, deproteinized by heating in boiling water for 60 s, and centrifuged before being applied to silica gel TLC plates. Two-dimensional TLC was used according to Brenner *et al.*¹² with the first run in methanol:chloroform:17% NH_3 (40:40:20) and the second run in phenol:water (75:25). Alanine was determined spectrophotometrically after elution of the spots as described by Chen and Diem¹³. The results are shown in Table 2, from which it seems that anaerobic beetles contained about three times more alanine than beetles kept aerobically.

The present study indicates that adults of *P. borealis* may survive long periods in nitrogen at 0°C in the laboratory. In these conditions lactate and alanine are accumulated but no accumulation of pyruvate, α -glycerophosphate and glycerol takes place. Further investigations should be carried out to see if other pathways of anaerobic metabolism are present, as in other insects^{2,4,5}. At present

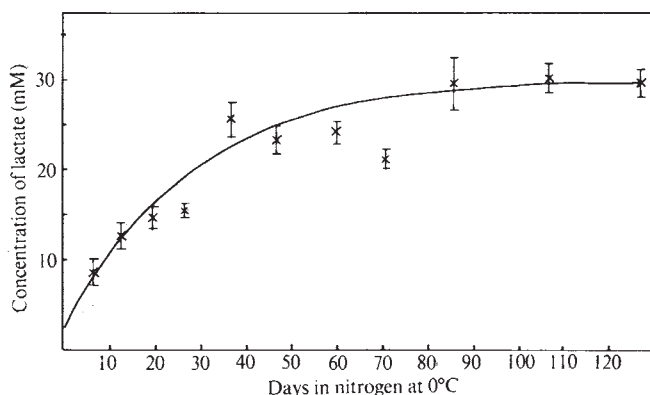


Fig. 3 Accumulation of lactate in the haemolymph of *P. borealis* stored in nitrogen at 0°C for various times. Means $\pm$ s.e. are given; each mean represents five to ten beetles.

Table 2 Comparison of Pyruvate α -Glycerophosphate and Alanine Contents in *P. borealis* Stored in the Laboratory in Aerobic and Anaerobic Conditions at 0° C for 45 to 70 d

		Pyruvate Fresh weight n† (mg g ⁻¹)	α -Glycerophosphate Fresh weight n† (mg g ⁻¹)	Alanine n† mmol
Aerobic	5	0.21 $\pm$ 0.017*	4 4.3 $\pm$ 0.27*	3 14.9 $\pm$ 2.28*
Anaerobic	5	0.17 $\pm$ 0.018	6 4.4 $\pm$ 0.18	3 51.3 $\pm$ 5.54

* Mean $\pm$ s.e.

† n, Number of replicates.

it seems important to stress the ecological significance of anaerobiosis in overwintering *P. borealis*. Beetles kept in laboratory conditions and those dug up from their overwintering sites both showed an "oxygen debt" and high contents of lactate in their haemolymph.

Overwintering adults of *P. borealis* may survive long periods of anoxia in their natural habitats. Although the habitats of *P. borealis* are exceptional in being flooded and covered by thick ice, other biotopes may offer similar conditions to overwintering insects. An oxygen deficiency is likely to occur in humid biotopes of various types when the ground is covered by ice and snow during the winter. Thus, anaerobic metabolism may be of vital importance for the survival of a number of insect species during overwintering.

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Cellular Organization and Fruit Ripening

Two major theories have been adduced through the years to explain the climacteric rise in respiration during the ripening of fruits. On the one hand, the climacteric has been imputed to a surge of protein synthesis¹⁻⁴. On the other, the climacteric has been attributed to a breakdown in "organization resistance"^{5,6}. Ethylene is considered as a causative endogenous ripening agent⁷.

The first view is based on observed increases in protein content and/or enzymatic activity in ripening fruit, as well as on the enhanced incorporation of amino acids into protein and nucleotides into nucleic acid during the climacteric^{3,4,8}. Nevertheless, a variety of observations offer persuasive reasons to

reject protein synthesis as a causative event in the initiation and full implementation of the climacteric, and possibly of ripening. The marked stimulation of glycolysis by anaerobiosis in pre-climacteric fruits (ref. 5, and manuscript in preparation), as well as the full respiratory activity of mitochondria isolated from them⁹ and the stimulating effects of uncouplers of oxidative phosphorylation on preclimacteric slices¹⁰, all suggest that preclimacteric fruit may have the full respiratory potential evinced by fruit at the height of the climacteric. Also, some fruits show no close correlation between respiration and protein synthesis¹¹.

The second view, which attributes the climacteric and ripening to the breakdown of organization resistance, was initially proposed as a speculation (ref. 5, and manuscript in preparation). There is little question that membrane permeability is markedly increased in ripe fruits^{12,13}. Permeability changes, however, need not be so gross in the early stages of ethylene presentation as to be perceptible as an increase in sucrose free-space (see Brady *et al.*¹⁴). Subtle permeability changes may have profound physiological effects¹⁵. We are re-examining the latter theory in terms of contemporary concepts of the central role of enzyme modulation in metabolic regulation. The study was engendered by observations of Barker *et al.*¹⁶ that the natural or ethylene-induced increase in glycolysis in bananas resembled the anaerobic stimulation of glycolysis in yeast and higher plants^{17,18}, in that the activity of two regulatory enzymes of glycolysis, phosphofructokinase and pyruvate kinase were enhanced in each case. In the light of a report that ethylene enhances glycolysis in yeast¹⁹, it was hoped that cyanide, a conventional evocator of aerobic fermentation in yeast, might stimulate glycolysis in fruits. As avocado slices possess a cyanide insensitive respiration²⁰, we anticipated that cyanide might also increase respiration, in conjunction with increased glycolysis, in intact preclimacteric fruits.

Low levels of cyanide in gaseous form were found not only to stimulate respiration but to cause ripening as well, so a general study was started to assess the possibility that the noted effects of cyanide and ethylene were on cellular organization in

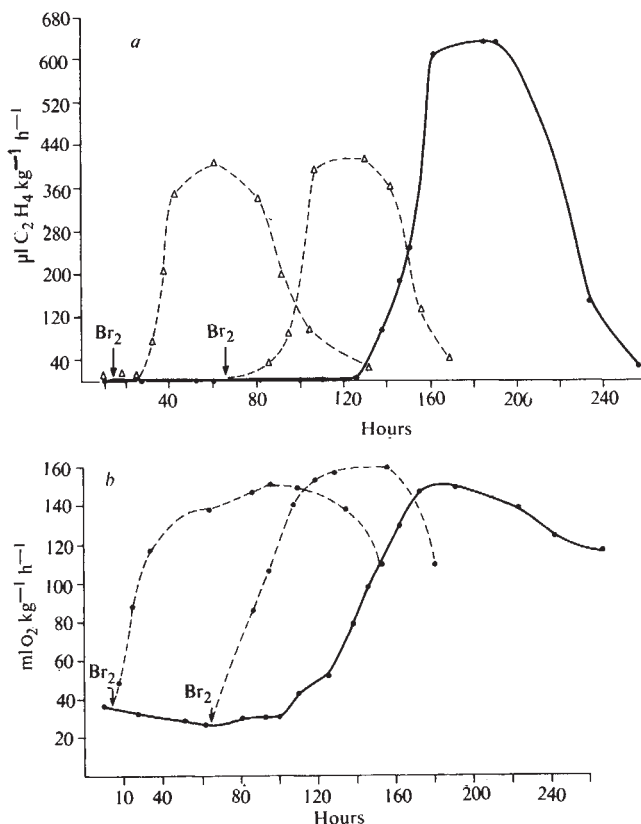


Fig. 1 a, Respiratory drift of intact avocados treated with 400 p.p.m. Br₂ vapours (---) or naturally ripened (control) (—); b, ethylene production by intact avocados treated as in a.

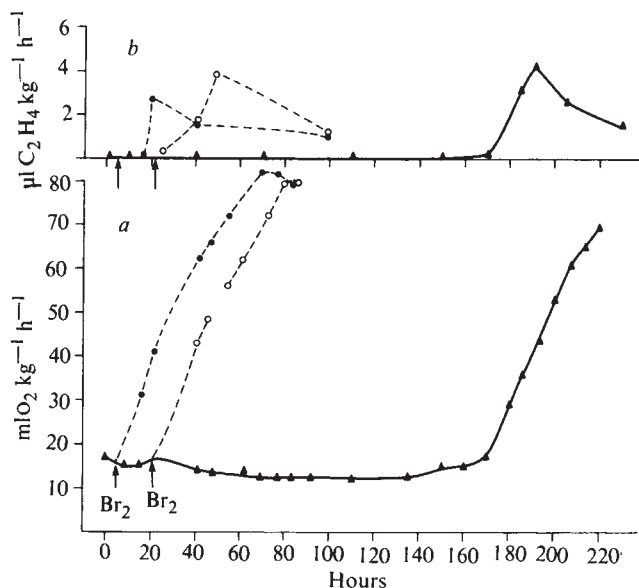


Fig. 2 *a*, Respiratory drift of intact bananas treated with 400 p.p.m. Br_2 (---) or 15 p.p.m. ethylene (—); *b*, ethylene production by intact bananas treated as in *a*.

general, and not on one or more specific enzymatic processes. We thus tried to determine whether simple compounds with different chemical characteristics, but with the potentiality of interfering with cellular organization in general, have an effect on the climacteric and on fruit ripening. We examined volatiles of several categories, as volatiles provide a convenient means for treating intact fruits. Halogens would be expected, among other things, to add to double bonds of the unsaturated fatty acids of phospholipids and thus alter the physical characteristics of cellular membranes, as has been reported with micro-organisms²¹. Chloroform, an effective lipid solvent, could be expected to influence cellular organization. The above compounds, as well as cyanide (400 p.p.m. in air) and CO (0.1% in air), caused a rapid onset of the climacteric, ethylene production and ripening of avocados and bananas. The experiments described below are limited to effects of Br_2 vapours on avocados and bananas.

When avocado fruits were exposed to Br_2 vapours (400–700 p.p.m. in an air stream) respiration rose at once, followed closely by an increase in ethylene production and subsequent fruit softening (Fig. 1*a, b*). The gas-ripened fruit proved indistinguishable from naturally ripened fruit with respect to peak values of respiration and ethylene production, and to texture. At similar concentrations, Br_2 caused a rise in respiration and ethylene production in preclimacteric bananas (Fig. 2*a, b*). The sugar content of bromine-ripened bananas was similar to that of ethylene-treated fruits (Table 1). Bromine soon turned the banana skins red, but had no effect on softening and sugar content. These agents elicit maturation changes similar to the outward changes in naturally ripened fruits, but it is an open question whether ripening induced by halogen gases, chloroform and cyanide resembles natural ripening in every detail. In addition to the induction of the climacteric all these agents evoke an increase in ethylene production and we cannot, therefore, establish with certainty whether they exert their effect on ripening independently or through ethylene.

The effects of ethylene are numerous and varied. There is no convincing evidence that ethylene is a specific enzyme modulator or interacts chemically with cellular components²². It has been suggested that ethylene exerts its influence on plant tissues by complexing with the metal prosthetic group of pivotal regulatory sites, and that CO, an ethylene mimic, acts similarly²³. Cyanide and CO complex with certain metals in appropriate conditions, but halogens and chloroform, which bear no chemical resemblance to ethylene and may not act as metal-complexing agents, duplicate ethylene in rapidly inducing the climacteric onset and

Table 1 Changes in Sucrose and Glucose Contents of Intact Bananas Treated with Bromine or Ethylene

Fruit No.	Treatment	Stage of ripeness	Glucose mol g^{-1}	Sucrose mol g^{-1}
1	None	Preclimacteric	166	58
2	None	Preclimacteric	127	43.7
3	None	Preclimacteric	132	37.9
4	None	Preclimacteric	119	37.8
5	15 p.p.m. ethylene	Climacteric	182	532
6	15 p.p.m. ethylene	Climacteric	120	460
7	15 p.p.m. ethylene	Climacteric	190	510
8	Bromine	Climacteric	256	527
9	Bromine	Climacteric	304	363
10	Bromine	Climacteric	303	421
11	Bromine	Climacteric	105	524
12	Bromine	Climacteric	190	419
13	Bromine	Climacteric	205	520

Charges expressed as mol g^{-1} fresh weight. Treatment as in Fig. 2.

rise in ethylene production. Further, ethylene and its mimics are effective in the induction of the climacteric and ripening in conditions which may preclude protein synthesis (see earlier). In addition, ethylene *in vitro* acts as a membrane perturber²⁴. For all these reasons we favour the interpretation that ethylene perturbs the cellular organization of fruits by physical means.

One of the consequences of the assumed perturbation is stimulation of glycolysis. This alone cannot, however, account for the climacteric rise and ripening (manuscript in preparation). Thus the perturbation of cellular organization must also influence aerobic respiration. The frequent rise in ATP in association with ripening favours this view. Sucrose formation and protein synthesis are frequent concomitants of ripening, the preponderance of one or the other depending on the fruit. There is a good argument to be made that sucrose formation and protein synthesis are the consequences of the climacteric rise, a reflexion of a surge in ATP synthesis resulting from the decontrol (not uncoupling) of respiration. In potato tubers, where ripening is not at issue, ethylene induces an increase in respiration which, depending on the physiological stage of the tuber, is followed by a large increase in sucrose content (manuscript in preparation).

Ethylene production follows an autocatalytic course during the climacteric rise. This has been attributed either to its influence on the *de novo* synthesis of^{3,4}, or to its catalytic influence on a metal-containing receptor centre of²⁵, one or more enzymes involved in ethylene biosynthesis. The present data, however, indicate that preclimacteric avocados and bananas have the enzymatic capacity for the production of biologically active amounts of ethylene, and that ethylene formation can be rapidly initiated by factors which have no metal-complexing properties, and in conditions which preclude protein synthesis (the presence of cyanide and uncouplers of oxidative phosphorylation). It seems, therefore, that the physical integrity of the cell may be an important parameter in regulating ethylene production in preclimacteric fruits. Once ethylene formation has been started it can stimulate its own production by its postulated ability to influence cell integrity.

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Reply to Criticisms of a Quantum-Mechanical Muscle Model

THE fundamental idea in a highly speculative new kind of muscle model¹ was that a bond vibration is first produced by an exothermic chemical reaction, the energy is transferred resonantly to an electronic excited state of identical frequency, then trapped in an excimer state formed between two such electronic oscillators and finally converted into external work as the two oscillators approach each other. The first part of Naqvi's criticism² was that the experiment I quoted as an example of energy trapping in excimers was mistaken. This I concede; clearly the pyrene experiments can no longer be used to show that excimer fluorescence is dipole-forbidden. The accepted theory clearly predicts, however, that, in special conditions, the excimer state may yet be long lived. In its longer version³ my muscle model already has a design feature which enables Naqvi's criticism to be overcome. If the oscillator was indeed first excited to a singlet state and only later diffused into interaction distance with another oscillator in the ground state then, as Naqvi argues, the triplet state would be at a lower energy and decay into it would be difficult to avoid. My suggestion³ was, however, that the energy of the bond vibration produced on ATP hydrolysis is less than that of the excited state of an isolated oscillator, so that only when the two oscillators are close enough to perturb each other sufficiently do they resonate with the bond vibration and then become excited. This feature was introduced, first, to protect the postulated bond vibration (by allowing it to be buried in a protein and yet to interact with the environment in this way) and, second, to ensure that only the anti-symmetric attractive excimer state is produced on excitation. I now suggest a third function: to ensure that the excimer only becomes excited when its energy is already below that of the triplet state. In this case decay into the triplet state would be prevented, and the fluorescence would become fully dipole-forbidden.

Naqvi also argues² that thermal motions must lower the symmetry of the excimer state, causing it to decay. This conclusion neglects the evolutionary capacity of a protein molecule. All that is necessary is that a protein should have evolved which holds the oscillators rigidly enough parallel and yet allows them to approach each other; such a structure

is easy to devise mechanically. Liquid dye lasers protect ions in their excited states from thermal decay by, in some cases, surrounding the ion with chelate cages to prevent random collisions; a protein may well do something similar.

The criticism of Banks, Callomon and Vernon⁴ was that the rapidity of vibrational relaxation in free solution makes it physically impossible to store the postulated bond vibration away from thermal equilibrium for the time necessary for the above model. Such relaxation is indeed a crucial difficulty in my muscle model, and one I have considered elsewhere³. The process of relaxation⁵ takes place on collision and the whole of the vibrational energy has to be converted in one step into a combination of alternative vibrational, translational and rotational modes. Thus the rate of relaxation depends critically on the energy gap between the relaxing vibration and the nearest mode (usually another vibration) beneath it. On this basis the theory of Schwartz, Slawsky and Herzfeld⁶ makes predictions which are consistent with experiments⁵. In extreme cases⁵, with large energy gaps, a vibration can last for as much as 10 s (N₂) or 6 s (CO), and not merely in dilute gases as Banks, Callomon and Vernon claim but at 1 atm and 0° C. In a liquid, relaxation is faster because of the 100-fold increase in collision rate, but its mechanism is the same. An answer to the problem therefore is to select (or evolve) an environment for a vibrating bond in which translational motions are cut to a minimum, rotations are prevented and neighbouring bonds are far from resonance. I am postulating^{1,3} that a special site has evolved in a protein for this function; this is by no means impossible. It is well known⁷ that proteins have hydrophobic cores and that enzymes need to hold substrates in such pockets to work at all. The figure of 10⁻⁷ s I suggested^{1,3} for the relaxation time was conservative. Apart from this point, my model depends less on the absolute lifetime of the state than on the relative rapidity of resonant energy transfer (~10⁻¹⁴ s). Thus there is no need to invoke new principles in this model, but only to make use of what is already well known in other fields.

Banks, Callomon and Vernon⁴ have also criticized my statement of the Second Law of Thermodynamics on the grounds that I was only led to make it because I assumed *a priori* that ATP can store energy. This was not the case; in fact I was led^{1,8} to restate the second law to solve the problem raised by Popper⁹, namely to find a statement consistent with Brownian motion and thus applicable at the molecular level. This was achieved⁸ by defining stored energy and heat relative to the cycle time of the machine which attempts to use these energies. In effect (but not, I think, explicitly) spectroscopists have been doing this for years. Experimentally there are many processes (such as fluorescence, phosphorescence, chemiluminescence and photosynthesis) that trap and store energy in single molecules¹⁰. I am merely suggesting that ATP stores energy in a similar way.

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BOOK REVIEWS

Modern Crystallography

X-ray Crystallography: An Introduction to the Theory and Practice of Single-crystal Analysis. By G. H. W. Milburn. Pp. 217. (Butterworth: London, December 1972.) £6.

Chemical Crystallography. Edited by Monteath J. Robertson. Consultant Editor A. D. Buckingham. (MTP International Review of Science.) Pp. 346. (Butterworth: London; University Park: Baltimore, 1973.) £10; \$34.50.

THESE two books aim to bring crystallographers up to date in the application of their art to the discovery of the arrangements of atoms in crystals: Milburn's book on the methods, Robertson's on the results in recent years.

Milburn describes the experimental methods, both photographic and photon counter, by which the positions and intensities of the many diffracted beams of X rays from a crystal are measured, and guides the would-be structure solver through the various interpretative methods which have been evolved to convert the set of experimental data into a three-dimensional electron density map revealing the positions of the atoms. His book is essentially a handbook for students learning the art, and to this end the arrangement of topics broadly follows the lines of an actual investigation.

The preliminary chapter on basic concepts is more in the nature of a summary of what is to be found in standard textbooks of X-ray crystallography than a thorough exposition. It is too short and condensed for beginners; there are not enough diagrams, and some of those present are not very well drawn. Students starting on the subject would therefore be well advised to go elsewhere for the foundations. The rest of the book, however, should serve the student well; it contains a great deal of practical advice which should smooth the path of investigators handling crystals and diffraction apparatus for the first time and attempting the exacting art of interpreting the mass of experimental measurements.

The treatment of interpretative methods—the extraction of structure factors from the measured intensities, the unravelling of vector maps, the use of phase relations between different diffractions to build up a sufficiently comprehensive set of phases to lead to an approximate structure, the use of anomalous scattering in noncentrosymmetric crystals, and the refinement of approximate structures—is adequate, though the arrangement of the material

is sometimes less than ideal. For instance, the section on refinement comes before that on phase-relation methods of structure-solving, not at the end where it would be expected; and a wave diagram to explain the breakdown of Friedel's law in noncentrosymmetric crystals comes, not at the beginning of the section on anomalous scattering, but halfway through it, after the principal application has been considered. In short, the essential information is all there, but not always arranged to form a smooth logical course of instruction. It is also noticeable that the author relies more on algebraic formulations than on diagrams; this is a matter of personal taste—it would suit some students but not others.

The book edited by Robertson is a collection of essay-reviews on selected topics of chemical interest based on the results of crystal structure determinations in recent years. All are written by well-known specialists in their respective fields, and together they present an impressive body of achievement, underlining the very great contributions X-ray (and electron and neutron) crystallography has made to our knowledge of molecular structure and of the significance of geometrical factors in determining the properties of molecules and crystals.

Most of the chapters consist largely of brief mentions of series of structures; they will obviously be of great value to others working in the same or related fields. The average reader's interest is more likely to be engaged by the resulting generalizations in theories of chemical bonding, or the properties in relation to stereochemistry, or the methods and strategy of crystal structure determination. J. C. Speakman's account of hydrogen bonding covers the history of ideas on the forces involved, and illustrates several aspects of the phenomenon by suitable examples. B. A. Frenz and J. A. Ibers, in a chapter on the structural chemistry of transition metal complexes, choose two topics from this large subject—5-coordination, and nitrosyl complexes—and discuss the degree to which the results could have been anticipated from current theory, or failing this, by analogy with other known structures not yet adequately covered by theoretical concepts. A survey of investigations of crystal structures in the USSR by B. K. Vainshtein and G. N. Tishchenko shows the wide range of structure types which engage the attention of Russian crystallographers. S. C. Abrahams deals with cooperative phenomena in inorganic

materials—magnetic ordering, dielectric properties, the only recently recognized phenomenon of ferroelasticity, and superconductive materials.

There are three chapters on substances of biological importance. A. L. Mathieson, in a chapter on natural products, selects from this huge field the terpenes and steroids. Marjorie C. Harding's review of large-molecule substances includes examples of porphyrins and corrins, penicillins, cephalosporins and other antibiotics, peptides, sugars and nucleotides. Protein crystallography is dealt with by T. L. Blundell and Louise N. Johnson in what is one of the best essays in the book; the subject forms a more connected story than those of most of the other chapters, it is written in an engaging style, and the discussion goes beyond structure description to the relation between structure and function.

The chapter on "direct methods" by J. Karle and Isabella L. Karle stands apart from the rest in that it reviews the structures which have been solved by a particular type of procedure which has been developed in recent years, largely by the Karles themselves. By this procedure, which uses phase relations between different diffractions, it is now possible to solve quite complex structures without the aid of heavy atoms as phase markers. The structures reviewed here form an impressive array, testifying to the power of the method.

The impression given by this volume is of an explosion of information gained by crystallographic methods; the increasing availability of automatic diffractometers and large computers, together with the phase relation methods mentioned in the previous paragraph, have increased the scope and very much accelerated the work involved in crystallographic methods, while the increasing use of anomalous scattering effects has revealed the absolute configuration of many dissymmetric molecules.

CHARLES W. BUNN

The Responsible Brain

The Conscious Brain. By Steven Rose. Pp. 351. (Weidenfeld and Nicolson: London, May 1973.) £4.95.

THIS is an idiosyncratic book, which is a good thing. It is also a book which seeks to expose some of the social repercussions of brain research, which is another good thing. Steven Rose's main interests in the past few years have been the biochemical and neurophysiological basis of the brain's adaptability, and the social and political aspects of

science. In case anyone should think from the title that this book is going to illuminate the neurological basis of consciousness, I should warn them. The title *The Conscious Brain* seems to serve not as an indication of the book's subject matter, but as a slogan expressing these interests of the author and his view that brain research is important but that the practitioners of this research should be consciously responsible for the consequences of their work.

The political and social targets are mostly fairly obvious: for example, the relationship between the extensive funding of work on synaptic function and the development of nerve gases, the drug scene, the IQ debate, the immorality of certain kinds of pharmacological or neurophysiological assaults on people, and society's attempt to neutralize the discontented or rebellious by categorizing their behaviour as mental illness, or labelling it with pseudo-scientific terms like minimal brain dysfunction. But for a book which has social comment as one of its major concerns these matters are treated rather too briefly and without much documentation.

Unfortunately, the parts of the book dealing with experiments and theories of brain mechanisms are even more disappointing. Nor does Rose's mode of literary expression help very much. We read that "Homo sapiens appears at the top of [the] league chart" of neuronal connexions, and that "even at 1.5 mm in length the embryo is gathering itself together for its spurt towards sentience". We are continually told that this or that piece of research is striking, dramatic, astonishing, remarkable, intriguing, tantalizing, and so on. What Rose often fails to do is to write about the research in sufficient detail, or with sufficient explication of its theoretical basis or implications to make it appear striking, intriguing or whatever to the reader, relying instead on the mere assertion. Vision, for example, is given the necessary couple of pages, with recognition of the letter "A" explained thus. "This is not the point at which to enter into the complex perceptual, psychological and philosophical arguments about what constitutes 'A-ness'. . . . But it seems reasonable to assume that there are present in the visual cortex classificatory cells, which are responsible both for the learning of a particular input—that of 'A', for example—and later for its recognition in a variety of guises". So much for vision. The next subject is the cerebellum, which we are told has been subjected to a remarkable piece of analysis (see appendix 3) or should that read (contd p. 94)?

The main point Rose wishes to make is that synapses and their modification provide the key to understanding the brain: "It is not too strong to say that

the evolution of humanity followed the evolution of the synapse". Further on, summing up his proposal that "learning represents the opening up of new functional pathways" and that these pathways or neural networks must be diffusely distributed, he remarks, "I do not think that there are any phenomena of memory which cannot be explained by this redundant network/modifiable synapse theory". The trouble is that there is not a great deal more discussion of what the phenomena of memory are than there was of the phenomena of perception, and nothing that constitutes an explanation of them. It all strikes one rather as if it had been asserted that there are no phenomena of literature that cannot be explained by the "ink marks on paper" theory.

In the final chapter Rose upbraids the holders of views which he regards as inadequate or, worse still, irresponsible: those who see man merely as an animal, those who see him as a machine, and those who argue against a rational basis for the mind. Again one looks forward to finding some fresh insight into these issues, but again one is a bit disappointed: for example, "In a sense it is because Skinner is nearly right in some things that he is so fundamentally in error overall". Few people will find this kind of rhetoric any more compelling in a book on science than they do in a political speech. It seems regrettable because Rose raises quite a lot of questions which do have real interest, and often there are substantial and important things to be said about them. Perhaps, then, this is a good book for those who like to think for themselves. They can fill in some of the substance that Rose leaves out.

KEITH OATLEY

Sea Mammals

Functional Anatomy of Marine Mammals. Edited by R. J. Harrison. Pp. xvii+451. (Academic: New York and London, December 1972.) £7.50.

THE past decade has seen a considerable expansion of studies of marine mammals, covering a wide range of disciplines. This volume is the first of a series and includes eight varied papers on functional anatomy. It is a "mixed bag" of papers, unconnected by an obvious common theme, except that they deal with marine mammals, so the reviewer's task is not easy.

The volume is prefaced by a rapid review of some historical landmarks in the study of marine mammals. An excellent account of growth and development by M. M. Bryden follows. It is a *tour de force* covering all groups of marine mammals and including detailed reviews of general growth, as well as tissue and organ development.

The seals are particularly interesting because they are born on land, but subsequently spend most of their lives in water. This switch of environments necessitates anatomical and physiological adjustments which involve changes in relative growth affecting such components as muscle, fat, blood and eyes.

J. E. King presents a detailed study of skull growth of southern elephant seals. The skulls of a number of other species are then compared and an attempt made to correlate deviations from the normal pattern with the animals' way of life.

More than a quarter of the book is taken up by a detailed review, by P. J. Morgane and M. S. Jacobs, of the comparative anatomy of the cetacean nervous system which is very specialized and unlikely to appeal to readers without a particular interest in comparative neurology. F. Ramprashed and others contribute an account of the anatomy of the phocid ear. Comparisons are made with man, with other seals and with whales, and adaptations to aquatic life are discussed. Seals emit sounds at ultrasonic frequencies and may use them in their hearing mechanism, but it has not yet been demonstrated that they employ this mechanism for echolocation. A particularly interesting complementary paper by C. A. Repenning is concerned with the problems of directional hearing by pinnipeds in water. F. J. Tarasoff considers the adaptation of the hind limbs of otters and seals to life on land and in the water and thermoregulation problems are defined and discussed.

Finally, Harrison and others contribute a lengthy account of reproduction and gonad appearance in some toothed whales. The data from more than four hundred small odontocetes are extremely valuable but this chapter is too discursive, and would have been shortened and improved by further analysis. Nonetheless much of this material is unique and it is an achievement to have brought it together.

The book is well produced, with some outstanding contributions, few typographical errors and clear illustrations. It should be in general libraries, but not many workers will be able to afford personal copies. R. M. LAWS

Particle Kinematics

Particle Kinematics. By E. Byckling and K. Kajantie. Pp. ix+319. (Wiley-Interscience: London and New York, April, 1973.) £7.50.

A VOLUME on particle kinematics is perhaps a little different in structure from the average book met by the physicist. Usually the development of one aspect of a subject, whilst dependent on some basic core material, is not of vital

consequence to other aspects of the subject. The serial nature of the material presented in this book is in striking contrast with this pattern. In consequence the authors have a more onerous task: the value of their work depends much, not on their most lucid chapters, but on their least. It is also demanding of the reader, for although the breadth of the subject matter is limited, the comprehension of one chapter is often essential to an appreciation of subsequent material.

The core of the subject—special relativity and the concepts of phase space—is summarily reviewed in the opening chapters. This is but a prelude to the bulk of the book which concerns itself with the application of these basic principles to two particle final state decays, through ever more complicated systems and culminating in chapters dealing with interactions involving multiple particle production and “inclusive” reactions where the final state particles are not completely identified. The work is concluded by a rather isolated chapter on numerical methods of phase space integration. Much of the content of the book is of quite recent origin and is well referenced throughout.

The development of the material is logical and satisfying. Each successive chapter deals with an increasingly complicated system and is neatly integrated with both its predecessor and successor. It is this thorough, systematic, “one-dimensional” development of the material which is both a disadvantage and yet its *raison d'être*. The text is usually clear but always concise and, in a few places, perhaps a little too abbreviated. Thus the student may sometimes find his comprehension of the subsequent development decidedly impaired. It is just this coherent approach, however, together with its inclusion of more recent work, which distinguishes it from the few other volumes written on the subject and demands its careful perusal.

The formal nature of the book leads to something of a dearth of examples in the text. This is partially relieved by a generous series of exercises associated with most sections of the work. It is not completely satisfactory, however, and many readers—especially the less theoretically minded—will wish that a little more assistance was provided in the use of the material discussed.

The authors have supposed that the book will be of value to senior undergraduate and postgraduate students. It is probable that such students have not specialized their studies and many of them will find the brevity of the introductory reviews of relativity and—especially—phase space, inadequate to prepare them for the succeeding chapters.

In fact the volume is essentially

directed at the student theoretical physicist, who will find this an elegant treatise—although he may be disappointed that spin has been entirely omitted from consideration. The experimentalist, on the other hand, will also find it a most rewarding book, although he may well prefer the less formal, less demanding (but less complete) approach of some earlier authors.

R. M. BULL

Social Anthropology

Anthropologists and Anthropology: The British School 1922–1972. By Adam Kuper. (Allen Lane: London, April 1973.) £3.50.

THIS book, purporting to be an intellectual history of British social anthropology over the last half century, is a disaster equalled in recent years only by Marvin Harris's *The Rise of Anthropological Theory* which it resembles in its lack of attention to facts and its superficial judgments. However, its success is assured, for it has a perky style and the way in which irksome complications are ironed out by massive generalizations will make it appear to the mediocre student a heaven-sent source of all that needs to be known on the subject. Herein lies the disaster.

The year 1922 is an important date in the development of British social anthropology, for it was the year in which both Malinowski and Radcliffe-Brown published their first field monographs. From then on one or other of these two figures dominated the subject in Britain until the Second World War, and both have had a lasting influence, for good or ill, on the subject. It is appropriate therefore that a large proportion of this work should concentrate on these two men, and there is an arguable case for a full-scale study of their work and influence. Dr Kuper comes nowhere near providing such a study, and one might compare it with Luke's recent intellectual biography of Durkheim in order to show this.

The author's failure to deal adequately with the interwar period when social anthropologists were so few is not a good omen for his attempts on the postwar years during which the number of practising anthropologists grew enormously. Indeed his difficulties become worse and worse, and he is left with either making little more than a catalogue of names and titles of books or resorting to sweeping statements in which vital differences are submerged beneath a typology that includes such gross terms as the “Cambridge school”, “Manchester school” and “Oxford structuralists”. The author also sees the “professoriate” as one of the most important influences on the development of social anthropology but if his ethnography of university departments

were a little better he would have found that the variation in their organization makes the notion as meaningless as would be using chiefship in a similar way.

Another curious feature of the book is Dr Kuper's oddly anthropological approach; so closely has he drawn the boundaries round the community he is studying that it appears to be an isolated tribal society that exists in neither space nor time. There is little attempt to examine the external intellectual influences at work on members of this community. To take but one example, nowhere is there any indication of the tremendous influence and stimulus that Lévy-Bruhl's work was to Evans-Pritchard. The author's failure to allow for this and for other influences results in his presenting Evans-Pritchard as a far more Radcliffe-Brownian than a balanced approach would allow. For example, there is Evans-Pritchard's quite explicit criticisms of Radcliffe-Brown in 1928, and this from an individual who for years made his criticisms implicit.

This book is not simply bad for the students who will rely on it, but it does a disservice to the subject, for the outsider who reads it would have to assume that social anthropology was devoid of intellectual content. PETER RIVIÈRE

Inorganic Spectra

Inorganic Infrared and Raman Spectra. By S. D. Ross. Pp. xiii+414. (McGraw Hill: London and New York, September 1972.) £8.

THIS book consists of two parts, the first of which gives a résumé of the relevant theory, and the second and longer part of which is a review of the literature on the vibrational spectra of inorganic compounds. There are also appendices (group character and correlation tables and so on) and extensive bibliographical and formulae indexes. The first two chapters of part I outline point-group theory and the Wilson FG matrix method of frequency calculation. A feature of the book is the amount of attention devoted to crystal spectra, and accordingly part I has a third chapter sketching site-group and factor-group analysis and methods of calculating lattice frequencies. The author disclaims any intention of writing a textbook, and indeed the necessary compression of material in part I makes it unlikely to be fully comprehensible to the uninitiated. The outline of theory is also open to criticism in that, despite the author's declared interest in symmetry determination, it does not mention the degree of depolarization of Raman lines or infrared dichroism.

Siebert's similar book (1966) reviewed the literature up to a little beyond 1964,

and the second edition of Nakamoto's book (1970) brought the coverage up to the end of 1967. Now Dr Ross has extended it by a further three years. In accordance with his interest in crystal spectra, the first section of part II concerns compounds which must be regarded as having no discrete molecular vibrating unit. Succeeding sections deal with molecules and ions containing up to eight atoms, or in some cases more. The literature references (1,916 in number) are listed according to first authors, and there is a formula index arranged according to number of atoms and formula type. Researchers in this field will find it useful to have a literature review of this kind in a single book. However, the first four volumes of the *Chemical Society Specialist Reports (The Spectroscopic Properties of Inorganic and Organometallic Compounds)* have also reviewed the literature up to the same date as has Dr Ross. As long as the Chemical Society series continues, little call is likely for further books of the type which Dr Ross has written.

His book is excellently printed and produced. The sole misprint noticed (preface, page xi) raised a smile, for it led to the statement that rotational modes for which the rotations are not free are called "liberations" (*sic*).

L. A. WOODWARD

Nonserial Problems

Nonserial Dynamic Programming. By Umberto Bertele and Francesco Briroschi. Pp. xii+235. (Academic: New York and London, September 1972.) \$13.95.

This book deals with a combined dynamic programming and graph theoretic approach to optimizing the sum of functions of a set of variables which can take discrete values. The simplest type of problem referred to is

$$\min_X \{f(X)\} = \min_X \{\sum_{i \in T} f_i(X^i)\}$$

where $X = \{x_1, x_2, \dots, x_n\}$, $T = \{1, 2, \dots, n-1\}$, $X^i = \{x_i, x_{i+1}\}$.

This is referred to as the serial unconstrained optimization problem which is well known in the dynamic programming area. It is serial because the $\{x_i\}$ can be completely ordered and the dynamic programming formulation involves only one state variable at each stage. It is, in fact,

$$F_k(x_k) = \min_{x_{k-1}} \{f_k(x_{k-1}, x_k) + F_{k-1}(x_{k-1})\} \quad n \geq k \geq 2$$

$$F_1(x_1) = f_1(x_1)$$

with $\min_{x_n} \{F_n(x_n)\}$ required.

The authors do not actually state this equation but the elimination process used is equivalent. First of all x_1 is eliminated by optimizing over x_1 when x_2 is fixed. Then x_2 is eliminated and finally x_n is eliminated.

The main purpose of the book is to

generalize the above problem to ones in which X^i does not take the above form. Thus, for example, we might have

$$X = \{x_1, x_2, x_3, x_4, x_5\};$$

$$X^1 = \{x_1, x_2, x_3\};$$

$$X^2 = \{x_2, x_3, x_4\};$$

$$X^3 = \{x_3, x_4, x_5\}$$

Such a problem is a nonserial problem.

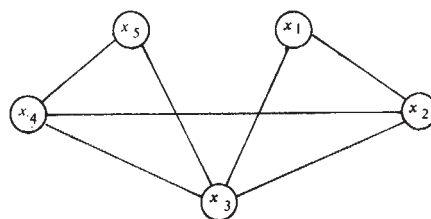
A similar dynamic programming elimination process is used, but in this case, when a variable is eliminated its interactions with other variables have to be considered. Thus we see that: x_1 interacts with x_2 and x_3 because they appear in at least one of the component functions; x_2 interacts with x_1, x_3, x_4 because it appears in component functions containing these variables; similarly x_3 interacts with x_1, x_2, x_4, x_5 , while x_4 interacts with x_2, x_3, x_5 and x_5 interacts with x_3, x_4 . In solving this problem we can eliminate x_1 first of all giving rise to the optimization problem

$$\min_{x_1} \{f_1(x_1, x_2, x_3)\}$$

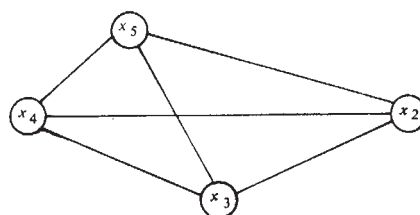
which is a function of x_2 and x_3 , say, $h_1(x_2, x_3)$. The original problem is then reduced to a similar optimization problem to the original with three component functions in variables x_2, x_3, x_4, x_5 . Having eliminated x_1 , if we now eliminate x_2 , it interacts with x_3 and x_4 and we find $\min_{x_2} \{h_1(x_2, x_3) + f_2(x_2, x_3, x_4)\}$ which is a function h_2 of x_3 and x_4 . The process is repeated. A formal dynamic programming formulation exists, although the authors only do this implicitly in their calculations.

A further extension is to include constraints on some of the variables.

Now the above interaction concept has clear analogies with graphical methods and the following is an interaction graph of the above problem; which is self explanatory.



If we eliminate x_3 first this is equivalent to deleting x_3 and joining up all the others which interact with x_3 . Thus we have



The process can be repeated until the graph is reduced to a single vertex.

So far this is fairly routine. The central problem to which the book addresses itself is the one of determining the best elimination sequence from a computational point of view. The amount of computation at each stage (that is, elimination graph) depends both on the degree of a vertex (that is, number of interactions for that vertex for the specific elimination graph) and the number of values each vertex can take. The book considers specific measures of computational effectiveness and makes great use of the concept of dominance. Thus if $d = (d_1, d_2, \dots, d_n)$ and $d' = (d'_1, d'_2, \dots, d'_n)$ are the degrees of the vertices, in the order in which they are eliminated (that is, d_i does not necessarily mean the i^{th} variable), and relative to the elimination graph reached at that stage, then d dominates d' if $d_i \leq d'_i$ for all i and $d_i < d'_i$ for at least one i . Dominance plays an important role in a special class of computational effectiveness functions, $\mathcal{J}$.

The authors then proceed to examine the problem of determining good elimination orderings as a problem in graph theory. The theorems considered are given constructive proofs so that particular properties of specific graphs can be made use of, in a clearly defined manner, in finding such orderings within the class $\mathcal{J}$.

It is possible to formulate the secondary computational problem as an optimal route problem, with its own graphical representation. The authors do this, but do not make use of it, for it is very easy to find the secondary problem more computationally burdensome than the primary problem.

In essence the previous paragraphs capture the general spirit of the book. Additional aspects are special consideration of parametric problems (that is, those in which particular variables are specified to take values in a given range, and the solutions are required relative to each such specification) for which dynamic programming has obvious advantages, and an extension from single variable elimination to block variable elimination, with corresponding treatment of the graph theoretic computational measures as with the single variable elimination process. A limited amount of consideration is given to practical applications.

The book is very well written and amply illustrated. The basic ideas are easy to follow, and, for the less mathematically inclined, an attempt to follow the ideas via the illustrations themselves would be profitable. The full comprehension of the interpretation and proofs of the theorems requires a good background in horticultural graph theory (flowers, bushes, trees, forests and so on) which are, however, defined in the appendix.

D. J. WHITE

CORRESPONDENCE

Tungus Black Hole?

SIR,—The interesting suggestion of Jackson and Ryan that the Tungus event was caused by a black hole (*Nature*, **245**, 88; 1973) has a flaw. If the postulated interstellar population of small black holes inhabits the Galaxy, their mass density is $\geq 10^6 M_{\odot} \text{pc}^{-3}$ (assuming a flux of 1 per century over the Earth), which is impossible. A cosmological habitat is equally impossible. Implausible alternatives are: (1) Those objects are confined to the solar system (why? gathered somehow during formation?); their total mass is $\geq 10^{-2} M_{\odot}$. (2) These objects are much rarer, and the Earth is most fortunate to have received a visit in the last century. (3) One can somehow make as large a bang with a much smaller black hole.

Yours faithfully,

DOUGLAS M. EARDLEY

W. K. Kellogg Radiation
Laboratory,
California Institute of Technology,
Pasadena,
California 91109

Tungus Event

SIR,—In regards to an objection raised by Mr Eardley to the black hole explanation of the Tungus event, I should point out that we do not make a claim for there being a flux of 1 per century over the Earth. In fact the only estimate of substellar mass black hole mass density is that of Hawking and others that it may be on the order of the cosmological mass defect.

A more serious question is why the object would have a velocity characteristic of the solar system. Can this be due to a complex perturbative encounter of the small mass with the Earth-Moon system? Would this explain the strange discrepancy in the longitude of the Moon for the period of 1900 to 1920¹?

Yours faithfully,

A. A. JACKSON

Department of Physics,
The University of Texas at Austin,
Austin,
Texas 78712

¹ Munk, W. H., and MacDonald, G. J. F., *The Rotation of the Earth* (Cambridge University Press, 1960).

Transcription Unit

SIR,—During the proceedings of the Thirteenth International Congress of Genetics in Berkeley (August 20–29, 1973) it became apparent that there is now a definite need to have operational terms for the units of transcription and translation, particularly in eukaryotic systems. I suggest the terms 'transcon' and 'translon' for the units of transcription and translation respectively. A transcon—as presently understood by the work of people like Richard Firtel of the Massachusetts Institute of Technology, Cambridge, and Brian McCarthy of the University of California, Santa Barbara—includes that segment of the total DNA sequence on a chromosome which is located between two spacer sequences; a spacer sequence consists of about twenty-five thymine-adenine base pairs (poly(A) in mRNA). A transcon has a total length of about 30,000 base pairs in insects and has alternating sets of repetitive and unique sequences of DNA. A transcon thus has several structural genes (unique sequences) and is obviously different from a replicon in eukaryotes or an operon in prokaryotes. A translon may be defined as the unit of translation at mRNA level. It may be as small as a single gene, for example in the case of haemoglobin mRNA, or as large as a hundred or so genes, for example, perhaps, in the case of histone mRNA.

Yours faithfully,

SURESH C. GOEL

Department of Zoology,
University of Poona,
Poona 7

Reprints for USSR

SIR,—I have just today received a request from Professor E. M. Nadgornyi of the Institute for Solid State Physics of the Academy of Sciences of the USSR in Moscow for a reprint of one of my scientific articles as well as for exact details of special search techniques for digital computer simulation.

While requests of this sort are not infrequent, in this instance I hesitated to send the information requested because of the recent increased repression of Soviet advocates of civil rights and of Soviet scientists who have applied for permission to emigrate to Israel.

Because of the immediate dismissal of leading Soviet Jewish scientists and engineers from the professional positions and their virtual scientific ostracism following such visa applications, I decided to adopt a different approach in sending the requested information in this particular instance.

I have written to Professor E. M. Nadgornyi and informed him that while I personally favour cooperation, collaboration, and dissemination of information between scientists of all countries, I cannot condone the official attitudes of the Soviet government in respect of such noted scientists as Professors Lerner, Levich, and Sakharov. I am, therefore, sending all of the information requested by Professor Nadgornyi regarding special computer simulation techniques to Professor Aleksander Lerner, a noted cyberneticist and computer scientist. I am informing Professor Nadgornyi that he can obtain these documents by contacting Professor Lerner, a fellow Muscovite.

I feel that this may be a very important method by which scientists and engineers in the western world can make it clear to their counterparts in the USSR that such repression of Soviet Jewish scientists and, indeed, of many Soviet civil rights activists such as Sakharov and Solzhenitsyn is counter to any East-West *detente*, be it political, cultural, or scientific. I therefore urge others to follow my example in this matter of transmitting reprints and other information to the USSR.

Yours faithfully,

MARC HERBERT RICHMAN

Division of Engineering,
Brown University,
Providence,
Rhode Island 02912

Diving Terns

SIR,—Dr Dunn's observations on the diving success of terns¹ are very interesting, but I think do not take account of what may be the most important fish behaviour contributing to their surviving the unwelcome attentions of such birds of prey. I refer to the "Mauthner Reflex", the startle-response of teleost fish, which is discussed in detail elsewhere². A bird in free fall from a height of 10 m would, after hitting the water,

take some 40 ms to arrive at a fish swimming 0.5 m below the surface (longer than this if the impedance of the water is allowed for). The shock wave set up by the bird, however, would reach the fish in considerably less than 1 ms, and in a good-sized fish the sequence: activation of the VIII nerves $\rightarrow$ activation of Mauthner neurones $\rightarrow$ activation of motor neurones $\rightarrow$ onset of tail flip, would take up not more than 15 ms. Thus the almost inevitable reflex movement of the fish would certainly have begun at the time the bird arrived, and on its completion the fish would be displaced by an amount approximately equal to its own length from the position it occupied when the bird hit the water. ("Purposive" escape-swimming would usually follow the success of such an evasive but automatic reflex response.)

Whether or not the bird catches the fish would be very much influenced by the depth of the fish, the speed of the bird hitting the water, the momentum and direction of its continued dive, and possibly optical stimuli to the fish, etc.; the latter could, in theory, slightly advance the moment of excitation of the two Mauthner cells in the hind-brain, or influence which fired first, and thus which way the fish moved. Yasargil and I have shown³ that the interval between the firing of these cells is the all-important factor in eliciting the reflex response. When both cells fire simultaneously or within about 150 μ s of each other, no tail flip ensues; a mutual crossed inhibitory mechanism operates at every segment of the spinal cord. Any factors which would make the excitability fluctuations (the synaptic noise) in the two Mauthner cells out of phase would increase the probability of the reflex being produced and *vice versa*. Perhaps relatively calm conditions bias the situation in favour of the former probability, and thereafter with increasing windspeed the situation is reversed, the in phase noise component would be increased, the two Mauthner cells would fire within the critical 150 μ s period, and the tail flip would not operate. However, the other suggestions made by Dunn are probably valid, and it seems likely that the characteristics of the Mauthner reflex are among a number of factors which affect the survival chances of the fish, albeit the most important.

Yours faithfully,

JACK DIAMOND

Department of Neurosciences,
McMaster University,
Hamilton, Ontario L8S 4J9

¹ Dunn, E. K., *Nature*, **244**, 520 (1973).

² Diamond, J., in *Fish Physiology*, *The Mauthner Cell* (edit. by Hoar, W. S., and Randall, D. J.), 5 (Academic, New York and London, 1971).

³ Yasargil, G. M., and Diamond, J., *Nature*, **220**, 241 (1969).

Announcements

University News

Dr A. Ozin, University of Toronto, has been awarded the Meldola Medal and Prize for 1972 of the Royal Institute of Chemistry.

Drs M. J. Owen and R. G. Rose, University of Nottingham, have been awarded the 1972/73 Novadel Prize.

Appointments

Professor A. D. Baddeley, University of Stirling, has been appointed Director of the Medical Research Council's Applied Psychology Unit.

Sir Michael Clapham has been re-appointed Chairman of the Council for National Academic Awards. The following have been appointed members of the Council: Sir David Barran; Mr S. T. Broad; Dr A. H. Chilver; Dr Patricia H. Clarke; Mr D. Davis; Professor L. A. Gunn; Mr T. McL. Howie; Dr Ian M. Mackintosh; Professor G. D. S. MacLellan; Professor D. G. MacRae; Dr R. G. Murray; Mr T. B. Owen; Professor A. M. Ross; Miss A. C. Shrubsole; Professor J. E. Salmon; Mr S. W. Smethurst; Mr K. S. Toft; Rev G. Tolley; Mr G. R. Tyler; Professor M. Zinkin.

The following have been appointed members of the Nuclear Power Advisory Board: Lord Aldington; Mr A. E. Hawkins; Sir John Hill; Sir Peter Menzies; Dr A. W. Merrison; Mr R. V. Moore; Mr J. R. S. Morris; Lord Penney; Mr F. L. Tombs.

Miscellaneous

The Headquarters Office of the Public Health Laboratory Service Board has moved to Lower Entrance, Colindale Hospital, Colindale Avenue, London NW9 5EQ. (01-205 1295.)

Ramsay Memorial Fellowships have been awarded to the following: Mr H. J. L. Clarke, University of Strathclyde; Dr Hajime Kato, Cambridge University; Mr Jose Gonzalez, University of Sheffield.

Corrigendum

In the article "New Antibacterial Agent via Photofluorination of a Bacterial Cell Wall Constituent" by J. Kollonitsch *et al.* (*Nature*, **243**, 346; 1973) the first sentence in paragraph 5 should read "The assigned structure is in agreement with: (a) PMR spectrum (60 MHz; D₂O/DCI): two multiplets (CH₂) centred at 5.3 p.p.m. (J_{H-F}: 47); two multiplets (CH) centred at 4.65 p.p.m. (5.02 and 4.28 p.p.m.)."

Errata

In the article "Embryonic and Adult Chymotrypsins of Chicken Pancreas", by A. Cohen and R. G. Kulka (*Nature*, **244**, 97; 1973), line 3 of the legend to Fig. 1 should read "... chymotrypsinogens from a, 19-d-old embryo . . ." instead of "... 10-d-old embryo . . .".

In the article "Immunological Relationship of DNA Polymerase from Human Acute Leukaemia Cells and Primate and Mouse Leukaemia Virus Reverse Transcriptase", by G. J. Todaro and R. Gallo (*Nature*, **244**, 206; 1973), the symbols were omitted from the legend to Fig. 3 and should read as follows: "●, Effect of antibody (IgG) prepared against reverse transcriptase of gibbon ape leukaemia virus on acute leukaemic cell polymerase; ▲, effect of control sera (pre-immunized animal) on human leukaemic cell polymerase; ○, effect of antibody (IgG) prepared against reverse transcriptase of FeLV on human leukaemic cell polymerase."

International Meetings

November 1-3, **Magnetospheric Cleft Symposium** (American Geophysical Union, 1707 L Street, N.W., Washington D.C. 20036).

November 1-6, **The Royal Society Meetings** (The Royal Society, 6 Carlton House Terrace, London SW1Y 5AG; 01-839 5561, ext. 278).

November 2-4, **Management for Quality** (The Meetings Secretary, The Institution of Metallurgists, Northway House, High Road, Whetstone, London N20 9LW).

November 3, **Geologic Data Analysis with Computers** (Dr D. F. Merriam, Department of Geology, Syracuse University, Syracuse, New York 13210, USA).

November 4-7, **Transport, Survival and Fertilizing Ability of Spermatozoa** (Professor C. Thibault, Station de Recherches de Physiologie Animale, INRA, 78350 Jouy-en-Josas, France).

November 5-6, **Biochemical Society Meeting** (The Meetings Officer, The Biochemical Society, 7 Warwick Court, High Holborn, London WC1R 5DP).

November 5-6, **Forward Planning in the Service Sectors** (Forward Planning Symposium, Science Policy Foundation, Benjamin Franklin House, 36 Craven Street, London WC2N 5NG).

November 5-7, **Normal and Osteoarthritic Cartilage** (The Secretary, Professorial Clinical Unit, Institute of Orthopaedics, Stanmore, Middlesex HA7 4LP).

November 8, **Analytical Chemistry in the Polytechnics** (Society for Analytical Chemistry, Analytical Division, Chemical Society, 9/10 Savile Row, London W1X 1AF).

November 8-9, **First Annual National Conference of the Institution of the Rubber Industry** (Mr. J. Islip, Tyre Compound Laboratory, Tyre Technical Headquarters, Fort Dunlop, Erdington, Birmingham B24 9QT).

November 8-13, **21st International Meeting of Communications and Transports** (Segreteria Generale I.I.C., Villa Piaggio, Via Pertinace, 16125 Genova, Italy).

November 9, **Science Economics and Maritime Technology 1750-1850** (The Education Department, National Maritime Museum, Romney Road, Greenwich, London SE10 8NF).

November 9, **Symposium on the Mathematics of Telecommunications Traffic** (The Secretary, Institute of Mathematics and its Applications, Maitland House, Warrior Square, Southend-on-Sea, Essex SS1 2JY).

November 12-14, **Evaluation of Product Performance** (The General Secretary, Society of Cosmetic Chemists of Great Britain, 56 Kingsway, London WC2B 6DX).

November 13-15, **Cryotech '73** (The Lawson Organisation, Green Dragon House, 64 High Street, Croydon CR9 2UH).

November 13-16, **19th Annual Conference on Magnetism and Magnetic Materials** (F. B. Hagedorn, Conference Chairman, Bell Laboratories, Murray Hill, New Jersey 07974, USA).

November 13-18, **Interocean '73** (Chairman of the Organising Committee, o. Prof. Dr.-Ing. C. Kruppa, Technische Universität Berlin, Institut für Schiffstechnik, 1 Berlin 10, Salzuffer 17/19; telephone 03 11/3143411).

November 14, **The Revised Medical and Dental Code of Practice** (J. R. A. Lakey, Publicity Secretary, British Radiological Protection Association, c/o Royal Naval College, Greenwich, London SE10).

November 15, **Fatigue Behaviour of Composite Materials** (The Meetings Officer, Institute of Physics, 47 Belgrave Square, London SW1X 8QX).

November 15, **Fatigue in Composites** (Dr B. Harris, Applied Sciences Laboratory, Falmer, Brighton BN1 9QT).

November 15-16, **Electron Diffraction for the Investigation of Structure** (The Meetings Officer, The Institute of Physics, 47 Belgrave Square, London SW1X 8QX).

November 15-16, **Childhood Obesity** (Stella Klemper, Institute of Human Nutrition, College of Physicians and Surgeons, Columbia University, 511 West 166th Street, New York 10032).

November 16-17, **Genetical Society Meeting** (Dr Derek A. Smith, Genetics Department, University of Birmingham, PO Box 360, Birmingham, 15).

November 19-22, **7th British Insecticide and Fungicide Conference** (Conference Secretary, Mr W. F. P. Bishop, Frank

Bishop (Conference Planners) Ltd, 87 London Road, Croydon CR0 2RF; 01-688 4115).

November 20-22, **EURIM, A European Conference on Research into Management of Information Services and Libraries** (The Conference Organiser, Aslib, 3 Belgrave Square, London SW1X 8PL).

November 20-22, **An Analytical Symposium** (Barrington G. Lloyd, Press Officer, Pye Unicam Ltd, Cambridge; 0223-58866, ext. 369).

November 21-24, **14th Seminar on Electrochemistry** (The Director, Central Electrochemical Research Institute, Karakudi 623003, Tamil Nadu, India).

November 22, **Residual Stress Analysis** (Dr B. Singh, Fulmer Research Institute Ltd, Stoke Poges, Slough, Bucks. SL2 4QD).

November 22-23, **Advances in Avian Physiology** (Dr Malcolm Peaker, The Zoological Society of London, Regent's Park, London NW1 4RY).

November 23, **7th Croxson Memorial Lecture** (Mr T. M. Calvert, c/o R. H. Harry Stanger, Summerfield House, Barnet Lane, Elstree, Herts).

November 24, **The Biological Basis of Deer Management** (Dr D. Chapman, 'Larkmead', Barton Mills, Bury St Edmunds, Suffolk IP28 6AA).

November 27-30, **5th International Symposium on Basic Environmental Problems of Man in Space** (The Secretary, International Academy of Astronautics, 250 Rue Saint-Jacques, 75005 Paris, France).

November 28, **Nicolaus Copernicus, 500th Anniversary** (Dr T. J. Blachut, Head, Photogrammetric Research, NRC, Physics, M-36, Montreal Road, Ottawa, Ontario K1A 0S1).

November 29, **Review of New Developments in the Diagnosis and Management of the Hypertensive Patient** (Office of the Director, The University of Texas Health Science Center at Houston, Division of Continuing Education, PO Box 20367, Houston, Texas 77025, USA).

November 29-30, **Chauffage Electrique et Mieux-Etre** (J. B. Daublain, Le Secrétaire General, Comité Français d'Electrothermie, 25 Rue de la Pepiniere, Paris-8e, France).

Reports and Publications

not included in the *Monthly Books Supplement*

Great Britain and Ireland

The Soil Survey of England and Wales: Annual Report for 1972. Pp. 46. (Harpenden, Hertfordshire: Soil Survey of England and Wales, Rothamsted Experimental Station, 1973.) [186]

National Museum of Wales. Eggs of British Birds. By Colin Matheson and James A. Bateman. Sixth edition. Pp. 24. (Cardiff: National Museum of Wales, 1973.) [186]

Building Research Establishment Digest No. 154: Local Housing Programmes. Pp. 8. (London: HMSO, 1973.) 5p. [186]

Overseas Development Institute. Annual Report for 1972. Pp. 39. (London: Overseas Development Institute, 10-11 Percy Street, W1, 1973.) [196]

Innovation and Human Risk: The Evaluation of Human Life and Safety in Relation to Technical Change. By Craig Sinclair, Pauline Marstrand and Pamela Newick. (Science Policy Research Unit, University of Sussex.) Pp. 36. (London: Centre for the Study of Industrial Innovation, 162 Regent Street, 1972.) £1. [206]

Department of the Environment, London. Welsh Office, Cardiff. Local Government Finance in England and Wales: Consultation Paper. Pp. 6. (London: Department of the Environment; Cardiff: Welsh Office, 1973.) [216]

The Committee of Vice-Chancellors and Principals of the Universities of the United Kingdom. A Compendium of University Entrance Requirements for First Degree Courses in the United Kingdom in 1974-75, excluding degree courses for External Students. Pp. 325. (London: The Association of Commonwealth Universities, 1973. Published for the Committee of Vice-Chancellors and Principals.) £1.80. [216]

Research Using Transplanted Tumours of Laboratory Animals—a Cross-Referenced Bibliography—IX. By D. C. Roberts and M. Barton. Pp. 213. (Mill Hill, London: Registry and Information Service for Experimental Tumours, Research Data Unit, Imperial Cancer Research Fund, 1973.) [226]

Greenwich Time Report—Royal Greenwich Observatory. Time and Latitude Service—1972, July-September. Pp. 297-312. (London: Science Research Council, 1973.) [226]

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Potato Marketing Board. Survey of Potato Research in the United Kingdom 1973. Pp. 83. (London: Potato Marketing Board, 1973.) [27]

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PEP Broadsheet No. 542: Personal Mobility and Transport Policy. By Mayer Hillman with Irwin Henderson and Anne Whalley. Pp. vi+134. (London: Political and Economic Planning, 1973.) £2. [27]

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Inland Waterways Association. 1973 Rally of Boats, Ely—3 to 6 August 1973. Organized by the IWA Cambridge Section. Pp. 60. (London: The Inland Waterways Association, 1973.) 20p. [37]

Bulletin of the British Museum (Natural History). Entomology. Vol. 28, No. 1: The Genus *Etiella* Zeller (Lepidoptera: Pyralidae): a Zoogeographic Study. By P. E. S. Whalley. Pp. 1-21+15 plates. £2.60. Geology. Vol. 21, No. 6: Mid-Tertiary Cytherettinae of North-West Europe. By M. C. Keen. Pp. 259-349+23 plates. £4.60. Vol. 23, No. 3: A Review of Some English Palaeogene Nassariidae, Formerly Referred to *Cominella*. By C. P. Nuttall and J. Cooper. Pp. 177-219+9 plates. £2.50. Zoology. Vol. 24, No. 9: Miscellaneous. Pp. 423-474. £2.30. Vol. 25, No. 1: On the Cichlid Fishes of the Genus *Pelmatochromis* with Proposal of a New Genus for *P. conicus*; on the Relationship Between *Pelmatochromis* and *Tilapia* and the Recognition of *Saethodon* as a Distinct Genus. A New Species of Cichlid Fishes of Rivers Quanza and Bengo, Angola, with a List of the Known Cichlidae of These Rivers and a Note on *Pseudocrenilabrus Natalensis* Fowler. By E. Trewavas. Pp. 1-37+1 plate. £1.65. Vol. 25, No. 4: Biology and Fine Structure of *Euglypha Rotunda* (Testacea: Protozoa). By R. H. Hedley and C. G. Ogden. Pp. 119-137+7 plates. £1.60. Vol. 25, No. 5: A Revision of the *Haplochromis* and Related Species (Piscis: Cichlidae) from Lake George, Uganda. By P. H. Greenwood. Pp. 139-242. £4.55. (London: British Museum (Natural History), 1972 and 1973.) [47]

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Swedish Natural Science Research Council. Bulletins from the Ecological Research Committee, No. 18: Scandinavian Aerobiology. Edited by Siwert Nilsson. Pp. 222. (Stockholm: Swedish Natural Science Research Council, Wenner Gren Centre, Box 23136, 1973.) [216]

Molecular Biology Reports, Vol. 1, No. 1, June 1972. Editor-in-Chief: H. Bloemendal. Pp. 1-74. Subscription price per volume of 500 pages Dfl.170 (US\$59.50), including postage. Private persons Dfl.75 (US \$25.50). (Dordrecht, Holland; Boston, USA: D. Reidel Publishing Company, 1973.) [216]

United States Department of the Interior: Geological Survey. Bulletin 1336: Precambrian Geology of the Northern Bradshaw Mountains, Yavapai County, Arizona. By C. A. Anderson and P. M. Blacet. Pp. v+82+2 plates. Professional Paper 800-C: Geological Survey Research 1972, Chapter C. Pp. v+281. \$3. (Washington, DC: Government Printing Office, 1972.) [226]

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International Union for Conservation of Nature and Natural Resources. IUCN Publications, New Series, No. 28: Papers and Proceedings of IUCN's Twelfth Technical Meeting, Banff, Alberta, Canada, 12 to 15 September 1972. Edited by Hugh F. I. Elliott. Pp. 383. (Morges, Switzerland: International Union for Conservation of Nature and Natural Resources, 1973.) \$6. [57]

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European Brewery Convention. Report of the Barley Committee—Field Trials 1970 (Malting 1971). Pp. lxxxi+249. (Zeist, The Netherlands: National Institute for Malting Barley, Malt and Beer, 1973.) [57]

Smithsonian Contributions to Zoology, No. 147: The Old World Stenomidae—a Preliminary Survey of the Fauna, Notes on Relationships, and Revision of the Genus *Ertogenes* (Lepidoptera: Gelechioidea). By W. Donald Duckworth. Pp. 21. (Washington, DC: Smithsonian Institution Press, 1973. For sale by US Government Printing Office.) 30 cents. [67]

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